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## THE PILOT CHART OF THE NORTH ATLANTIC OCEAN,

ISSUED MONTHLY BY THE UNITED STATES HYDROGRAPHIC OFFICE.*


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[*A Lecture delivered before* THE FRANKLIN INSTITUTE, *Friday, January 27, 1888.*]

Mr. HAYDEN was introduced by the Secretary of the INSTITUTE, and spoke as follows:

*Ladies and Gentlemen, Members of the* INSTITUTE:—The pilot chart, which I have the honor to describe to you this evening, is very different in character from all other charts with which the

* This lecture was illustrated by twenty-four lantern slides, and such illustration is very essential to clearness. Through the courtesy of the hydrographer, Commander J. R. Bartlett, U. S. N., we are enabled to issue a copy of the chart itself, however, which will give great assistance to the reader. W.

navigator has to deal. It is, I may say, a *composite* chart; that is, an ordinary track chart, with the addition of a great deal of timely, interesting and useful data, which do not appear on any other chart. The object is, first, to supply a reliable chart upon which to plot a vessel's track after getting clear of the coast (where, of course, more detailed and larger scale charts are used); and, secondly, to present graphically any information relating to the North Atlantic of interest and value to mariners during each month of the year. It would require far more time than I have at my disposal this evening thoroughly to illustrate how this is accomplished month after month and year after year, and I shall only attempt to describe a single edition of the Pilot Chart, the one for January, 1888.* In doing so, I propose to follow first the synthetic method, starting with the outline, or base chart, and adding other data, just as is done in the actual publication of the chart. This will, I think, make the subject far clearer to an audience, most of whom, I take it, are not familiar with all the technical details relating to charts and navigation. Then, after having briefly described the general character of the chart and the different kinds of data which are published on it, I shall make a hasty and necessarily somewhat superficial analysis of the chart, discussing successively such features as may seem most interesting and instructive.

To begin with, I will show you the base of the Pilot Chart, which is simply a track chart of the North Atlantic on Mercator's Projection.† This comprises what may be called the permanent portion of the chart, or the portion which does not change from month to month, but is the same each month of the year. This is lithographed in black by means of a transfer from a copper plate. Near the top you will notice the compass card, which the navigator uses to lay off his course. In the lower left-hand corner is a storm card (Northern Hemisphere), which illustrates the circulation of the wind around an area of low barometer, with brief practical rules for action to avoid the dangerous portions of an approaching cyclone. At the right-hand side is given an

* Although the January chart was described in the lecture, the chart for March is published herewith, necessitating certain slight changes in the lecture itself, as printed.

† To follow the text, refer to the Pilot Chart, published herewith, considering first what is printed in *black*, then in *blue*, and finally in *red*.

explanation of symbols. The symbols which are thus explained are those used in plotting other data on this base, as will be explained in a few moments. The light curved lines which cross the chart are variation curves; that is, curves showing the variation of the magnetic compass. You will notice, also, the very light dotted line near the coast: that is the 100-fathom curve. The small arrows, which, indeed, are so very fine on the original chart as to hardly show when reduced to the very small size required for use with a lantern, indicate the general drift of ocean currents. Although these currents vary considerably during the year, it is only safe to indicate their general direction or average for the entire year; to attempt to do more would merely lead to error. Without speaking further at present of this outline or base chart, I will take up next what are called the "blue data." I will say here, by the way, that in order to avoid confusion, and to make the chart as clear as possible in spite of all that is printed upon it, two colors are used for the second and third impressions, respectively—blue and red. Practical difficulties make it impossible to show on this screen the base chart and the blue and red data just as they appear on the chart itself, superposed one upon the other, and I shall therefore be obliged to illustrate each separately from the base chart.

The blue data consist essentially of a meteorological forecast for the month following the date of issue, and in addition to this forecast there are plotted the principal steamship and sailing routes recommended for the month. The small circles and arrows which you see plotted uniformly over the ocean indicate graphically the probable percentage of calms, and the frequency and force of the prevailing winds in each five-degree ocean square. There being no fixed meteorological stations on the high seas, it is necessary to group together observations made on board vessels in some way by which they can be localized and averaged up; this is done by dividing up the ocean into squares bounded by five degrees of latitude and longitude, and every vessel which goes through one of these squares and keeps meteorological observations adds just so much to our knowledge of the prevailing weather conditions in that square. It therefore happens that there are many squares whose meteorological conditions are very well-known, on account of the very great number of vessels which

traverse them; while, on the contrary, there are other squares, which lie off the tracks of commerce, whose meteorological conditions are only approximately known. To repeat, therefore, the figures in the circles in each five-degree ocean square indicate the percentage of calms likely to be met with; the arrows fly with the wind and indicate by their length the relative frequency of prevailing winds, and by the number of cross-bars the force of the wind according to Beaufort's scale. This scale of wind force is a scale in general use among mariners for recording the force of the wind. It varies from 0 (a calm) to 12 (a hurricane); and though necessarily somewhat rough—the force of wind being estimated largely from the amount of sail a full-rigged ship can carry—yet from long practical usage it has become tolerably definite and accurate, and the figures of the Beaufort scale give a sailor an instant and vivid idea of the force of wind experienced. In addition to this graphic representation of the frequency and force of prevailing winds in each ocean square, there is printed a brief forecast and a table showing the normal reading of the barometer, arranged in tabular form by ocean squares. The double dotted line which you see up towards Newfoundland is the probable limit of the region of frequent fogs for the coming month. The dotted lines across the lower portions of the chart indicate the limits of the trade winds; that is, both limits of the northeast trades, and the northern limit of the southeast trades, the other limit of the southeast trades being south of the equator, and not appearing on this chart. Where these northeast and southeast trades meet there is the region of equatorial rains, indicated by the blue belt of irregular shape lying principally to the northward of the equator.

With this brief glance at the blue data, I will now refer briefly to the red data. In a general way it may be said that the red data comprise information collected during the month preceding the date of issue. On the ocean there are plotted the latest reported positions of derelict vessels, wrecks, and drifting buoys. The dotted line, where there is a dotted line, indicates the drift which each wreck has followed since it was first reported. There are also plotted the positions where whales and water-spouts were reported during the previous month, and the red belt off Newfoundland indicates the region where frequent fogs were encountered. In the lower right-hand corner is printed a brief weather

review of the preceding month, written at the last moment before going to press, but necessarily more or less incomplete, so far as the entire Atlantic is concerned. Above, there is, as you will notice, a large amount of printed matter, comprising a list of notices to mariners issued during the previous month, dangerous obstructions to navigation along the coast, charts published and cancelled, transatlantic steamship and sailing routes, the latest reported positions of logs from the big lumber raft which was abandoned off Nantucket, and various other matter likely to be of timely interest. To one who is not familiar with the subject it will seem almost impossible to publish on one chart such a variety of information of such a diverse character, and yet have a chart that can be of practical use in plotting a vessel's track. It would be very difficult to do so without this distinction of colors, and yet it has been done, to a certain extent, in the preparation of what is called the "Abstract" of the Pilot Chart, a reduced copy printed entirely in black expressly for the use of the *New York Herald*, *Boston Post*, and certain other papers which devote special attention to the interests of commerce, in order that they may publish it, reduced in size, in their regular edition the first day of each month. This being, as I have said, entirely in black and white, can be illustrated entire on the screen, and while it by no means comprises all the data published on the regular edition of the Pilot Chart, yet it serves as a very good illustration of the general effect of the chart with all the data printed on one sheet. For the sake of clearness it is here necessary to omit many things, the tracks of derelict vessels for instance, the arrows showing the general drift of ocean currents, the 100-fathom curve, storm card, regions of frequent fog, all the printed matter, etc.; still, the leading features of the chart are given very clearly and graphically.

To illustrate still better the amount of data which the regular edition contains, I have had a copy printed entirely in black, and it may be of interest to see just what it looks like printed in this way.*

Having made this brief and hasty synthesis of the chart, I will take up certain points in greater detail, and endeavor to analyze and discuss them at greater length. In the first place, let us resume

* This was illustrated on the screen and discussed, but need not be referred to further here.

our consideration of the track chart which serves as the base upon which the Pilot Chart, as it is finally issued, is built up. This chart is on what is known as "Mercator's Projection," devised by a celebrated Dutch geographer and mathematician named Kauffman (in Latin, Mercator). It has been in use among navigators, since the middle of the sixteenth century and still holds the foremost place as a practical working chart, in spite of all the improvements and refinements which three centuries of progress have suggested. There are some who think that it is a pity that such is the case, and that there are other and better projections which sailors ought to use in preference to the Mercator; nevertheless, the fact that this projection has held its own, and is now almost universally preferred, must undoubtedly be regarded as showing that the practical advantages which it possesses for use aboard ship are more than enough to counter-balance whatever theoretical objections may be urged against it. The great feature of a Mercator chart is, that the track of a ship steering a true course, describing the well-known loxodromic curve on the surface of the sphere, is here plotted as a *straight line*, and the true course to be steered in order to reach a given port may be taken directly from the chart. One of the principal *objections* to a Mercator chart is, that a great circle course, the shortest distance between any two points on a sphere, is represented on this chart by a *curved* line in every case except when a vessel is moving east or west on the equator itself, or when steering due north or south. To illustrate this, I will show you what is called a *gnomonic* chart, upon which a *great circle* is a *straight line*, while a *true course* is a *curved line*. The Mercator chart may be described as a projection of the surface of the sphere on a cylinder tangent to the earth at the equator. Although this is not strictly true, yet it is sufficiently close to the truth not to involve any appreciable error for our present purposes. A gnomonic chart, however, is a projection of the surface of the sphere on a plane tangent to the sphere—in this case, tangent at the intersection of the 30th meridian and the equator. Now, anyone can see that it is impossible to represent a curved surface on a plane surface without some distortion, and the only question is how to have the necessary distortion in such a form as to cause the least practical inconvenience. It is evident that the farther you get from the equator on this Mercator projection, the greater the

distortion, and thus we see that the ten degrees of latitude from the equator to 10° N. Lat. are not much more than half as long as the ten degrees between 50° and 60° N. Lat., at the top of the chart. The scale must, therefore, vary as you go north from the equator, and to measure a given distance on the chart you must refer to that part of the scale at the side of the chart which is in the latitude of the line whose length you wish to measure, the result being given in geographical miles (sixty to a degree of longitude measured on the equator). To illustrate the difference between the appearance of a great circle track on the gnomonic and the Mercator chart, suppose we take the case of a great circle from the Straits of Florida to the north of Ireland. On the gnomonic chart it is evident enough that a straight line, which is here a great circle, from the Straits of Florida to the north of Ireland passes close to Cape Race, the southeastern extremity of Newfoundland. Now, looking at the Mercator chart, it is here clearly evident that a straight line from the Straits of Florida to the north of Ireland passes about ten degrees, or 600 miles, to the southeastward of Cape Race. Therefore, a great circle plotted on the Mercator chart from the Straits of Florida to the north of Ireland would be a curved line starting in a northeasterly direction, approximately parallel to the coast of the United States, and curving close to Cape Race and across the Atlantic to the north of Ireland. When long distances are to be traversed, the great circle route is in general to be preferred on account of its being a shorter distance, especially in the case of steamships, as the weather likely to be encountered is not a matter of such great importance as it is to sailing vessels. To plot the great circle course on a Mercator chart, it is therefore necessary either to work it out by spherical trigonometry, or else to get it practically from the gnomonic chart by drawing the straight line which there represents the great circle track, and then transferring it to the Mercator chart by plotting the positions where the curve crosses certain meridians, and joining the points thus obtained by a curved line.

The variation curves, which I merely mentioned at first, are of great importance to mariners. The compass upon which the sailor has to depend is subject to many errors, the chief of which are *variation* and *deviation*: that is, the magnetic needle rarely points to the true North, but in a direction to the right or left of North,

according to its error at the time and place. The *deviation* of the compass comprises those errors which are *local* in their character; that is, due to the effect of immediately surrounding objects, such as the magnetism of the ship itself—and this is sometimes very great in an iron ship. The *variation* of the compass varies with the position of the ship, as shown by these curves of variation. Thus from Cape Race to New York, the variation of the compass changes from 30° W. to less than 10° W.; and from Cape Race to New Orleans, from 30° W. to more than 5° E., the *agonic* line, or line of *no variation*, being indicated by the heavier double line stretching from the coast near Charleston down through Puerto Rico and the Windward Islands to the northeastern coast of South America. To illustrate these variation curves more clearly, I will present a chart, covering a much wider area than the Pilot Chart, upon which variation curves are plotted for each degree, instead of for each five degrees as on the Pilot Chart. This illustrates very strikingly the positions of the *magnetic* poles of the earth, which do not by any means coincide with the geographic poles. On the contrary, there are two northern magnetic poles and two southern; up north of Hudson's Bay, at the point where these curves converge, there is one magnetic pole, and there is another to the northward of Siberia. Similarly there are two in the Southern Hemisphere, and these four poles of this great magnet, the earth, are constantly but slowly shifting their positions, and just so constantly and surely does the magnetic needle obey these varying but ever present forces, seldom pointing towards the pole which man has marked off on his artificial globe, but always true to the great natural laws to which alone it owes allegiance. The small figures with plus and minus signs, which you see at various places on this chart, indicate the yearly rate of change of variation, and this rate varies at different positions on the chart. Thus, near the Cape Verde Islands, it is plus $\frac{9}{10}$; here the variation increases $\frac{9}{10}$ of a minute a year; farther to the southward, near the South American coast, it is plus $7\frac{4}{10}$, and to the northward, near the Irish Channel, it is minus $7\frac{8}{10}$. Fortunately, however, these changes are small and comparatively regular, and their cumulative effect can be allowed for, when large enough to make it necessary to do so.

I stated at the beginning of my lecture that when near the coast large scale charts are used, giving greater detail, and to

illustrate, I will show you a copy of a chart recently issued by the Hydrographic Office, of Salinas Bay, west coast of Nicaragua. This bay is, I may say, in passing, near the terminus of the projected Nicaragua Canal, and we may yet see the day when it will have become famous. This shows very well the general character of such detailed charts, embracing the hydrography of the coast, and including the topography of the shore a short distance back from the coast line. The figures indicate the depth of water in fathoms at the position where each is plotted, and their meaning is rendered still clearer by the ten-fathom curve, five-fathom curve, and the sanded surface, where the depth is less than three fathoms. This question of *soundings*, and the *depth* of the sea, of vital importance near the coast, is also of great importance out in mid-ocean, far from land. Shoals are often reported at positions where the charts show no dangers to navigation; sometimes these reports are verified, but more often they are mistakes, and the charts were formerly covered with these doubtful dangers, to the never-ending annoyance of navigators. But the apparatus for deep sea sounding has been constantly improved, and our knowledge of the depths of the sea has been increased immensely by the combined efforts of the great maritime nations of the world. These doubtful dangers have thus been constantly reduced in number, and their existence disproved by our better knowledge of the depths of the sea. When a submarine cable is to be laid, the first question is as to the best route to follow, and a chart on which all these soundings are plotted is consulted and the question answered. Let me place before you a chart showing the topography of the bottom of the Atlantic. "The hills of the land stretch not so far as the billows of the sea; the heights of the mountains are not so great as the depths of the ocean."*

To resume consideration of the blue data, or meteorological forecast, let us take a glance at the general change of meteorological conditions during the year, very well illustrated by two charts taken from Berghaus' Physical Atlas, showing the isobars and winds for January and July. The most pronounced feature is

* The chart referred to was discussed briefly, but it need only be said here that the greatest depth is 4,561 fathoms, a sounding made by Lieutenant-Commander W. H. Brownson, U. S. N., to the northward of Puerto Rico.

the area of high barometric pressure to the southwestward of the Azores, and the permanent area of low pressure near Iceland. There is a greater difference of pressure in winter, the normal high barometer near the Azores being 768 millimetres, and the normal low pressure near Iceland being 747 millimetres, a difference of twenty-one millimetres, or ·85 inches; while in July the normal high barometer is 766 millimetres and the normal low barometer 757 millimetres, a difference of only ·36 inches. To this far greater difference of pressure in winter than in summer is due the greatly increased severity of storms over the North Atlantic during the winter season. The normal barometer over the continent of North America is much higher in January than in July, also, giving rise to the violent westerly and northwesterly gales which surge out over the North Atlantic during the winter season. There are shown, also, on these charts, the limits of the trade winds before spoken of, between which, and just to the northward of the equator, is the region of equatorial rains. The central line of this region has been well named the "Meteorological Equator;" it lies to the northward of the true equator and follows the sun as it changes its declination. "The great 'sun swing' of this calm belt," says Maury, "is annual in its occurrence; it marks the seasons and divides the year into wet and dry for all those places within the arc of its majestic sweep. But there are other subordinate and minor influences which are continually taking place in the atmosphere, and which are also calculated to alter the place of this calm belt, and to produce changes in the thermal status of the air which the trade winds move. These are, unusually severe winters or hot summers; remarkable spells of weather, such as long continued rains or droughts over areas of considerable extent. Either within or near the trade wind belt it is tremblingly alive to all such influences, and they keep it in continual agitation; accordingly we find that such is its state that, within certain boundaries, it is continually changing place and limits. This fact is abundantly proved by the speed of ships, whose log books show that it is by no means a rare occurrence for one vessel, after she has been dallying in the doldrums for days, in the vain effort to cross that calm belt, to see another coming up to her 'hand over fist,' with fair winds, and crossing the belt after a delay in it of only a few hours instead of days." Another authority, Mr. Robert H. Scott,

when speaking of these steady northeast trades, describes how, "when we reach the edge of the trade wind zone, we find the upper currents gradually descending to the sea level. On the peak of Teneriffe (latitude 28° N.), for instance, there are constant southwest winds at the summit. As the autumn comes on the sun moves southwards, the trade winds follow him, and the southwest wind creeps slowly down the mountain side until in winter the whole of the island, from summit to base, is enveloped in this return current, which in summer touches only the highest peak."

Such, then, is a general view of the great climatic changes which take place over the Atlantic as the seasons succeed one another, and the data plotted in blue on the chart indicate these changes graphically to the mariner, in a way to be taken practical advantage of in shaping his course. It was in this field that Maury's genius was so preëminent, and the practical benefits which his studies in marine meteorology have conferred upon commerce must give him as enduring a fame amongst the maritime nations of the world as that of Newton in the realm of science.

You are all more or less familiar, no doubt, with descriptions of the terrific winds and violent seas sometimes encountered by vessels in those terrible storms known as tropical cyclones. West Indian hurricanes, the name by which tropical cyclones in the North Atlantic are generally known, occur most frequently during the summer months, especially in August, and seem to have their origin, as a general rule, about the northern edge of the belt of equatorial rains, near the southern limit of the northeast trades and to the eastward of the Windward Islands. Starting as a whirlwind on a gigantic scale, they at first move bodily westward, sometimes crossing the Gulf of Mexico and reaching the coast of the United States with destructive violence, but more often curving to the northward and then to the northeastward, following up the Gulf Stream towards Newfoundland and circling around the area of permanent high pressure over the mid-Atlantic. Fortunately for sailors, the *direction of rotation* of the wind, which is drawn into and rushes furiously around the area of low barometer in one of these cyclones, is always the same; in the Northern Hemisphere it is invariably in a direction *against the hands of a watch*, as you look at a watch laid down with the face up; in the Southern

Hemisphere it is just the reverse. Upon this well-established fact, together with our knowledge of the progressive motion of the entire storm along a track whose general course is tolerably well known, is based the law of storms, which has been of invaluable assistance to navigators. During the summer months, therefore, special pains are taken to bring before navigators, in the most graphic and condensed form possible, the best rules for action in order to avoid the regions where these storms occur most frequently; or, if actually overtaken by one, in order to manœuvre to the best advantage. The chart which I now show on the screen represents the paths followed by a few West Indian hurricanes, and it will give a good general idea of their character and the general lines along which they move. The name of William C. Redfield, which appears on the title of this chart, is that of an American who is honored by every sailor and every meteorologist as the first thoroughly to grasp and explain the true character of these storms, and whose researches have been of incalculable benefit and have undoubtedly led to the saving of thousands of lives and millions of dollars worth of property at sea during the last half century. Now look at this chart and imagine the terrific winds of one of these tropical cyclones blowing about a centre of very low barometer in a direction indicated by the arrows, and this whole storm system moving along one of these tracks from low to high latitudes; at the centre, mountainous seas, and an almost inconceivable fury of the elements; and you will have a fair general idea of a West Indian hurricane.

During the winter season, although tropical cyclones occasionally occur, the greater portion of the storms which sweep the North Atlantic move along paths which cross the American continent from West to East—the same storms which you see charted on the daily weather map published by the Signal Service—moving over the great lakes, and down the St. Lawrence Valley towards Newfoundland; here they leave the coast, and, gathering renewed energy and violence, proceed on their destructive course towards northern Europe. Following these cyclonic storms come those violent westerly gales which make transatlantic steamship navigation so difficult, especially for westward-bound vessels, as those of you who have crossed the Atlantic at this time of the year can no doubt bear witness. In winter, therefore, it is the province of the

Pilot Chart to graphically forecast and explain *this* feature of ocean meteorology, as indicated by the next diagram, a reprint from the Pilot Chart for last December, with the addition of such material as was considered of great importance, and yet too much to add to the Pilot Chart itself without confusion, for even here we must draw the line somewhere. In this reprint we find discussed the winter storm belt of the North Atlantic and the transatlantic steamship routes recommended for December. Suppose, now, that we should attempt to generalize and summarize on a single chart, by some graphic method, the areas of greatest frequency of existence of barometric minima, or centres of more or less perfectly developed wind systems. Suppose, also, that we should attempt to plot on the same chart the mean paths followed by storms, or, technically speaking, of barometric minima, in order to see at a glance those regions where storms are most frequent, and the general paths which they pursue across the Atlantic. This has already been done for us, with characteristic care and fidelity, by the Deutsche Seewarte, the Hydrographic Office of the German Empire, and this chart I will now project upon the screen. These shaded areas are the regions of greatest frequency of barometric minima. The tracks indicated by heavier or lighter arrows are the paths along which storms travel with greater or less frequency. What a marked predominance to the northward, especially up towards that permanent area of low barometer near Iceland. Only one exception can be taken to this chart: it does not give sufficient prominence to the West Indian hurricane track; there were more of these tropical cyclones during two months last summer than are represented on this chart for an entire year. At the same time, the last hurricane season may be fairly said to have been one of exceptional violence.

Let us here pause for a moment and briefly review the ground that has been covered by my lecture thus far, and then as briefly outline what is yet to come: We have now considered successively the base of the Pilot Chart, the blue data, and the red data, first briefly and in a very general way, and then returning to the base and the blue data in order to enlarge upon each as far as possible, consistently with the limited time at our disposal. We shall next revert in a similar way to the red data, and finally consider briefly the methods by which reports are collected from masters

of vessels, and utilized in the preparation of the Chart; its publication, circulation and practical use; and the present condition and prospects of our commercial marine, to whose interests the work of the Hydrographic Office is so largely devoted.

(*To be continued.*)

## MACHINE DESIGNING.

By John E. Sweet.

[*A Lecture delivered before the* Franklin Institute, *Monday, January 30, 1888.*]

The Lecturer was introduced by the Secretary of the Institute, and spoke as follows:

*Ladies and Gentlemen, Members of the* Institute: "Carrying coals to New Castle," the oft-quoted comparison, fittingly indicates the position I place myself in when attempting to address members of this Institute on the subject of machine designing.

Philadelphia, the birthplace of the great and nearly all the good work in this, the noblest of all industrial arts, needs no help or praise at my hands, but I hope her sons may be prevailed upon to do in their right way what I shall try to do roughly—that is, formulate some rules or establish principles by which we, who are not endowed with genius, may so gauge our work as to avoid doing that which is truly bad. No great author was ever made by studying grammar, rhetoric, language, history, or by imitating some other author, however great. Neither has there ever been any great poet or artist produced by training. But there are many writers who are not great authors, many rhymsters who are not poets, and many painters who are not artists; and while training will not make great men of them, it will help them to avoid doing that which is absolutely bad; and so may it not be with machine designing? If there are among you some who have a genius for it, what I shall have to say will do you no good, for genius needs no rules, no laws, no help, no training, and the sooner you let what I have to say pass from your minds, the better. Rules only hamper the man of genius, but for us, who either from

choice or necessity work away at machine designing without the gift, cannot some simple ruling facts be determined and rules formulated or principles laid down by which we can determine what is really good, and what bad? One of the most important and one of the first things in the construction of a building is the foundation, and the laws which govern its construction can be stated in a breath, and ought to be understood by everyone. Assuming the ground upon which a building is to be built to be of uniform density, *the width* of the foundation should be in proportion to the load, the foundation should taper equally on each side, and the centre of the foundation should be under the centre of pressure. In other words, it is as fatal to success to have too much foundation under the light load as it is too little under a heavy one.

Cannot we analyze causes and effects, cost and requirements, so as to formulate some simple laws similar to the above by which we shall be able to determine what is a good and what a bad arrangement of machinery, foundation, framing or supports? A vast amount of work is expended to make machines true, and the machines, or a large majority of them, are expected to produce true work of some kind in turn. Then, if this be admitted, cannot the following law be established—that every machine should be so designed and constructed that when once made true it will so remain, regardless of wear and all external influences to which it is liable to be subjected. One tool-maker says that is right, and another that it cannot be done. No matter whether it can or cannot, is it not the thing wanted, and if so, is it not an object worth striving for? One tool-maker says that all machine tools, engines and machinery should set on solid stone foundations. Should they? They do not always, for in substantial Philadelphia some machine tools used by machine builders stand upon second floors, or, perhaps, higher up; and of these machine tools none, or few at least, except those mounted upon a single pedestal, are free from detrimental torsion where the floor upon which they rest is distorted by unequal loading. But to first consider those of such magnitude as to render it absolutely necessary to erect them —not rest them—on masonry, is due consideration always taken to arrange an unequal foundation to support the unequal loads?—and they cannot be expected to remain true if not. When one

has the good fortune to have a machine to design of such extent that the masonry becomes the main part of it, what part of the glory does he give to the mason? Is the masonry part of it always satisfactory, and is not this resorting to the mason for a frame rather than a support adopted on smaller machines than is necessary? Is it necessary even in a planing machine of forty feet length of bed and a thirty-foot table? Could not the bed be cast in three pieces—the centre a rectangular box 5, or 6, or 7 feet square, 20 feet long, with internal end flanges, ways planed on its upper surface, and ends squared off—a monster perhaps, but if our civil engineers wanted such a casting for a bridge they'd get it. Add to this central section two bevel pieces of half the length, and set the whole down through the floor where your masonry would have been and rest the whole on two cross walls, and you would have a structure that if once made true would remain so regardless of external influences. Cost? Yes; and so do Frodsham watches—more than "Waterbury." It may be claimed, in fact, I have seen lathes resting on six and eight feet, engines on ten, and a planing machine on a dozen. Do they remain true? Sometimes they do and many times they do not. Is the principle right? Not when it can be avoided; and when it cannot be avoided, the true principle of foundation building should be employed. * * * A strange example of depending on the stone foundation for not simply support, but to resist strain, may be found in the machines used for bevelling the edges of boiler plate. Not so particularly strange that the first one might have, like Topsy, "growed," but strange because each builder copies the original. You will remember it, a complete machine set upon a stone foundation, to straighten and hold a plate, and another complete machine set down by the side of it and bolted to the same stone to plane off the edge; a lot of wasted material and a lot of wasted genius, it always seems to me. Going around Robin Hood's barn is the old comparison. Why not hook the tool carriage on the side of the clamping structure, and thus dispense with one of the frames altogether?

Many of the modern builders of what Chordal calls the Hyphen Corliss Engine claim to have made a great advance by putting a post under the centre of the frame, but whether in acknowledgment that the frame would be likely to go down or the stone-

work come up I could never make out. What I should fear would be that the stone would come up and take the frame with it. Every brick mason knows better than to bed mortar under the centre of a window sill; and this putting a prop under the centre of an engine girder seems a parallel case. They say Mr. Corliss would have done the same thing if he had thought of it. I do not believe it. If Mr. Corliss had found his frames too weak, he would soon have found a way to make them stronger.

John Richards, once a resident of this city, and likely the best designer of wood-working machinery this country, if not the world, ever saw, pointed out in some of his letters the true form for constructing machine framing, and in a way that it had never been forced on my mind before. As dozens, yes hundreds, of new designs have been brought out by machine tool-makers and engine builders since John Richards made a convert of me, without anyone else, so far as I know, having applied the principle in its broadest sense, I hope to present the case to you in a material form, in the hope that it may be more thoroughly appreciated.

The usual form of lathe and planer beds or frames is two side plates and a lot of cross girts; their duty is to guide the carriages or tables in straight lines and carry loads resisting bending and torsional strains. If a designer desires to make his lathe frame stronger than the other fellows, he thinks, if he thinks at all, that he will put in more iron, rather than as he ought to think, how shall I distribute the iron so it will do the most good.

In illustration of this peculiar way of doing things, which is not wholly confined to machine designers, I should like to relate a story, and as I had to carry the large end of the joke, it may do for me to tell it.

While occupying a prominent position, and yet compelled to carry my dinner, my wife thought the common dinner pail, of which you are probably familiar (by sight, of course), was not quite the thing for a professor (even by brevet) to be seen carrying through the streets. So she interviewed the tinsmith to see if he could not get up something a little more tony than the regulation fifty-cent sort. Oh, yes, he could do that very nicely. How much would the best one he could make cost? Well, if she could stand the racket he could make one worth a dollar. She thought

she could, and the pail was ordered, made and delivered with pride. Perhaps you can guess the result. A *fac-simile* of the original, only twice the size.

Now this is a very fair illustration of the fallacy of making things stronger by simply adding iron. To illustrate what I think a much better way, I have had made these crude models (see *Fig. 1*), for the full force of which, as I said before, I am indebted to John Richards; and I would here add that the mechanic who has never learned anything from John Richards is either a very good or very poor one, or has never read what John Richards has written or heard what he has had to say.

Three models as shown in *Fig. 1* were exhibited; all were of the same general dimensions and containing the same amount of material. The one made on the box principle, *c*, proved to be fifty per cent. stiffer in a vertical direction than either *a* or *b*, from twenty to fifty times stiffer sidewise, and thirteen times more rigid against torsion than either of the others.

However strong a frame may be, its own weight and the weight of the work upon it tends to spring it unless evenly distributed, and to twist it unless evenly proportioned. For all small machines the single post obviates all trouble, but for machine tools of from twice to a half dozen times their own length the single post is not available. Four legs are used for machines up to ten feet or so, and above that legs various and then solid masonry. If the four legs were always set upon solid masonry, and levelled perfectly when set, no question could be raised against the usual arrangement, unless it be this: Ought they not to be set nearly one-fourth the way from the end of the bed? or to put it in another form: will not the bed of an iron planing machine twelve feet in length be equally as well supported by four legs if each pair is set three feet from the ends—that is, six feet apart—as by six legs, two pair at the ends and one in the centre, and the pairs six feet apart? There being six feet of unsupported bed in either case, with this advantage in favor of the four over the six, settling of the foundation would not bend the bed.

It is not likely that one-half of the four-legged machine tools used in this country are resting upon stable foundations, nor that there ever will be; and while this is a fact it must also remain a fact that they should be built so as to do their best on an unstable

one. Any one of the thousand iron planing machines of the country, if put in good condition and set upon the ordinary wood floors, may be made to plane work winding in either direction by shifting a moving load of a few hundred pounds on the floor from one corner of the machine to the other, and the ways of the ordinary turning lathe may be more easily distorted still. Machine-tool builders do not believe this, simply because they have not tried it. That is, I suppose this must be so, for the proof is so positive, and the remedy so simple, that it does not seem possible they can know the fact and overlook it. The remedy in the case of the planer is to rest the structure on the two housings at the rear end and on a pair of legs about one-fourth of the way back from the front, pivoted to the bed on a single bolt as near the top as possible.

A similar arrangement applies to the lathe and machine tools of that character—that is, machines of considerable length in proportion to their width, and with beds made sufficiently strong within themselves to resist all bending and torsional strains, fill the requirements so far as all except wear is concerned; that is, if the frames are once made true they will remain so regardless of all external influences that can be reasonably anticipated.

Among wood-working machines there are many that cannot be built on the single rectangular box plan—rested on three points of support. Fortunately the requirements are not such as demand absolute straight and flat work, because, in part from the fact that the material dealt with will not remain straight and flat even if once made so; and in the design of wood-working machinery it is of more importance to so design that one section or element shall remain true within itself, than that the various elements should remain true with one another.

The lathe, the planing machine, the drilling machine, and many other of the now standard machine tools will never be superseded, and will for a long time to come remain subjects of alteration and attempted improvement in every detail. The head-stock of a lathe—the back gear in particular—is about as hard a thing to improve as the link motion of a locomotive. Some arrangement by which a single motion would change from fast to slow, and a substitute for the flanges on the pulleys, which are intended to keep the belt out of the gear, but never do, might be improvements.

If the flanges were cast on the head-stock itself, and stand still, rather than on the pulley, where they keep turning, the belt would keep out from between the gear for a certainty. One motion should fasten a foot-stock, and as secure as it is possible to secure it, and a single motion free it so it could be moved from end to end of the bed. The reason any lathe takes more than a single motion is because of elasticity in the parts, imperfection in the planing, and from another cause infinitely greater than the others, the swinging of the hold-down bolts.

Should not the propelling powers of a lathe slide be as near the point of greatest resistance as possible, as is the case in a Sellers lathe, and the guiding ways as close to the greatest resistance and propelling power as possible, and all other necessary guiding surfaces made to run as free as possible?

A common expression to be found among the description of new lathes is the one that says "the carriage has a long bearing on the ways." Long is a relative word, and the only place I have seen any long slides among the lathes in the market is in the advertisements. But if anyone has the courage to make a long one they will need something beside material to make a success of it. It needs only that the guiding side that should be long, and that must be as rigid as possible—nothing short of casting the apron in the same piece will be strong enough, because with a long, elastic guide heavy work will spring it down and wear it away at the centre, and then with light work it will ride at the ends with a chattering cut as a consequence.

An almost endless and likely profitless discussion has been indulged in as to the proper way to guide a slide-rest, and different opinions exist. It is a question that so far as principle is concerned there ought to be some way to settle which should not only govern the question in regard to the slide-rest of a lathe, but all slides that work against a torsional resistance, as it may be called—that is, a resistance that does not directly oppose the propelling power; in other words, in a lathe the cutting point of the tool is not in line with the lead screw or rack, and a twisting strain has to be resisted by the slides, whereas, in an upright drill the sliding sleeve is directly over and in line with the drill, and subject to no side strain.

Does not the foregoing statement "that the propelling power should be as near the resistance as possible, and the guide be as near in line with the two as possible," embody the true principle? Neither of the two methods in common use meet this requirement to its fullest extent. The two-V New England plan seems like sending two men to do what one can do much better alone; and the inconsistency of guiding by the back edge of a flat bed is prominently shown by considering what the result would be if carried to an extreme. If a slide such as is used on a twenty-inch lathe were placed upon a bed or shears twenty feet wide, it would work badly, and that which is bad, when carried to an extreme, cannot well be less than half bad when carried half way.

The ease with which a cast-iron bar can be sprung is many times overlooked. There is another peculiarity about cast iron, and likely other metals, which an exaggerated example renders more apparent than can be done by direct statement. Cast iron, when subject to a bending strain, acts like a stiff spring, but when subject to compression it dents like a plastic substance. What I mean is this: if some plastic substance, say a thick coating of mud in the street, be levelled off true, and a board be laid upon it, it will fit, but if two heavy weights be placed on the ends, the centre will be thrown up in the air far away from the mud; so, too, will the same thing occur if a perfectly straight bar of cast iron be placed on a perfectly straight planer bed—the two will fit, but when the ends of the bar are bolted down the centre of the bar will be up to a surprising degree. And so with sliding surfaces when working on oil. If, to any extent elastic, they will, when unequally loaded, settle through the oil where the load exists and spring away where it is not.

The tool-post, or tool-holder that permits of a tool being raised or lowered and turned around after the tool is set, without any sacrifice of absolute stability, will be better than one in which either one of these features is sacrificed. Handiness becomes the more desirable as the machines are smaller, but handiness is not to be despised even in a large machine, except where solidity is sacrificed to obtain it.

The weak point in nearly all (and so nearly all that I feel pretty safe in saying all) small planing machines is their absolute weakness as regards their ability to resist torsional strain in the bed,

and both torsional and bending strain in the table. Is it an uncommon thing to see the ways of a planer that has run any length of time cut? in fact, is it not a pretty difficult thing to find one that is not cut, and is this because they are overloaded? Not at all. Figure up at even fifty pounds to the square inch of wearing surface what any planer ought to carry, and you will find that it is not from overloading. Twist the bed upon the floor (and any of them will twist as easy as two bass-wood boards) and your table will rest the hardest on two corners. Strap, or bolt, or wedge a casting upon the table, or tighten up a piece between a pair of centres eight or ten inches above the table, and bend the table to an extent only equal to the thickness of the film of oil between the surface of the ways, and the large wearing surface is reduced to two wearing points. In designing it should always be kept in mind, or, in fact, it is found many times to be the correct thing to do to consider the piece as a stiff spring, and the stiffer the better. The tooth of a gear wheel is a cast-iron spring, and if only treated as would be a spring many less would be broken. A point in evidence:

The pinions in a train of rolls, which compel the two or more rolls to travel in unison, are necessarily about as small at the pitch line as the rolls themselves; they are subject to considerable strain and a terrible hammering by back-lash, and break discouragingly frequent, or do when made of cast iron, if not of very coarse pitch, that is, with very few teeth—eleven or twelve sometimes.

In a certain case it became desirable to increase the number of teeth, when it was found that the breakages occurred about as the square root of their number, when the form was changed by cutting out at the root in this form (*Fig. 2*) the breakage ceased.

*a*. *Fig. 2*, shows an ordinary gear tooth and *b*, the form as changed; *c* and *d* show the two forms of the same width, but increased to six times the length. If the two are considered as springs, it will be seen that *d* is much less likely to be broken by a blow or strain.

The remedy for the flimsy bed is the box section; the remedy for the flimsy planer table is the deep box section, and with this advantage, that the upper edge can be made to shelve over above the reversing dogs to the full width between the housings.

The parabolic form of housing is elegant in appearance, but

theoretically right only when of uniform cross section. In some of the counterfeit sort the designers seem to have seen the original Sellers, remembering the form just well enough to have got the curve wrong end up, and knowing nothing of the principle have succeeded in building a housing that is absolutely weak and absolutely ugly, with just enough of the original left to show from where it was stolen. If the housing is constructed on the brace plan, should not the braces be straight, as in the old Bement, and the centre line of strain pass through the centre line of the brace. If the housing is to take the form of a curve, the section should be practically uniform, and the curve drawn by an artist. Many times housings are quite rigid enough in the direction of the travel of the table, but weak against side pressure. The hollow box section, with secure attachment to the bed and a deep cross beam at the top, are the remedies.

Raising and lowering cross heads, large and small, by two screws is a slow and laborious job, and slow when done by power. Counter-weights just balancing the cross head, with metal straps rather than chains or ropes, large wheels with small anti-friction journals, and the cross head guarded by one post only, changes a slow to a quick arrangement, and a task to a comfort. Housings of the hollow box section furnish an excellent place for the counter-weights.

The moving head, which is not expected to move while under pressure, seems to have settled into one form, and when hooked over a square ledge at the top, a pretty satisfactory form, too. But in other machines built in the form of planing machines, in which the head is traversed while cutting, as is the case with the profiling machine, the planer-head form is not right, both the propelling screw or whatever gives the side motion, should be as low down as possible, as should also be the guide.

There is a principle underlying the Sellers method of driving a planer table that may be utilized in many ways. The endurance goes far beyond any man's original expectations, and the explanation, very likely, lays in the fact that the point of contact is always changing. To apply the same principle to a common worm gear it is only necessary to use a worm in a plain spur gear, with the teeth cut at an angle the wrong way, and set the worm shaft at an angle double the amount rather than at 90°. Such a worm gear

will, I fancy, outwear a dozen of the scientific sort. It would likely be found a convenience to have the head of a planing machine traverse by a handle or crank attached to itself, so it could be operated like the slide rest of a lathe, rather than as is now the case from the end of the cross head. The principle should be to have things convenient, even at an additional cost. Anything more than a single motion to lock the cross head to the housing or stanchions, should not be countenanced in small planers at least. Many of the inferior machines show marked improvements over the better sorts, so far as handiness goes, while there is nothing to hinder the handy from being good and the good handy.

When we consider that since the post-drilling machine first made its appearance, there have been added Blasdell's quick return, the automatic feed, belt-driven spindles, back gears placed where they ought to be, with many minor improvements, it is not safe to assume that the end has been reached; and when we consider that as a piece of machine designing, considered in an artistic sense entirely, the Bement post drill is the finest the world ever saw (the Porter-Allen engine not excepted, which is saying a good deal), is it not strange that of all mechanical designs none other has taken on such outrageous forms as this.

One thing that would seem to be desirable, and that ordinary skill might devise, is some sort of snap clutch by which the main spindle could be stopped instantly by touching a trigger with the foot; many drills and accidents would be saved thereby. Of the many special devices I have seen for use on a drilling machine, one used by Mr. Lipe might be made of universal use. It is in the form of a bracket or knee adjustably attached to the post, which has in its upper surface a V into which round pieces of almost any size can be fastened, so that the drill will pass through it diametrically. It is not only useful in making holes through round bars, but straight through bosses and collars as well.

The radial drill has got so it points its nose in all directions but skyward, but whether in its best form is not certain. The handle of the belt shipper, in none that I have seen, follows around within reach of the drill as conveniently as one would like.

As the one suggestion I have to make in regard to the shaping machine best illustrates the subject of maintaining true wearing surfaces, I will leave it until I reach that part of my paper.

Perfect as are the modern boring machines, both horizontal and vertical (and the reason they average so good is probably because the imitators find no poor ones to copy), there is one thing that might in many cases be found valuable, and that is a rotary spindle driven at various speeds and fixed in a head movable along the cross beam of the vertical machine. This would be complete for boring the crank-pin hole in engine cranks, and a complete plain radial drilling machine, useful in all shops where the machine only has legitimate work enough to keep it employed for a part of the time. Such a machine could hardly be called a compound, and certainly not a combined machine, although used for turning, boring and drilling. Combined tools and machines are seldom successful. A common hammer, which may be said to have two business ends, a lathing hatchet, and Disston's nigger saw, as I believe we used to call it down South, cover nearly the whole ground in tools, and Scott Russell's idea, as expressed in his remark, after viewing a combined machine at the Paris Exposition, may be remembered to advantage. The machine referred to consisted of a central post with a drilling machine on one side and a slotting machine on the other. After staring at it with evident curiosity, the attendant somewhat elated at the apparent interest of his distinguished visitor, asked what he thought of it, to which he replied: "I was thinking if I had that machine which part I would cut off, the drill or the slotter?" What I know about a slotting machine is fairly comparable to Burton's knowledge of the French language. He said he knew the French language by sight, and that's about the extent of my knowledge of a slotter, and possibly this is the reason I could never persuade myself into the belief that they look well or as well as it is possible to make them. The slotting machine, like others of the overhanging class, that is drilling and punching machines, is subject to strains that they have to resist to a disadvantage, and where springing to a certain extent cannot be avoided. Might it not be set down as a principle that the framing should be devised so as to have the springing do the least possible harm. A slotting machine might be so designed that all the spring there was in the machine would have a direct tendency to throw the cut out of square with the table; whereas, if the sliding bar was worked with a walking-beam there would be no detrimental spring whatever. Evil resulting

in a punching machine from springing is of another character altogether; much springing adds immensely to the power required to do the punching, and unless specially designed to avoid such a result, the springing throws the punch out of adjustment with the die, and bad work and dull tools are the result.

If the upper and lower parts of a punching machine are just equally rigid or equally elastic, and the punch and die central between the two, then the springing, however much, would not throw the tools out of register, and would do no harm except the waste of power. The loss of power in springing a machine is sometimes returned when the spring returns, but in a punching machine all power expended in springing the frame is a dead loss. The springing of machine framing, the springing of tools, and the springing, all add to the cost of doing work, not only on account of a loss of power, but in the dulling of the cutters, and in this none more so than the milling machine. The tools or cutters themselves are costly, and the act of cutting, as performed by an ordinary slabbing cutter, differs from nearly all other cutting action. Each separate chip removed is a wedge-shape piece, and the cutting edge of the cutter starts in at the point of the wedge. The appearance of a milled surface, as ordinarily left on soft metal, is unusually more uneven than any feed or imperfection in the cutter accounts for, and it is safe to assume that the teeth of the cutters slip over more or less and then bite and gouge in deeper than if each one took its allotted amount. Both actions tend to dull the cutters, and not only the spring of the machine and cutters tend to produce this result, but an elastic feed motion also contributes to this same result, so that a rigid feed is likely more essential in a milling machine than in any other.

Many have thought that as a remedy for the difficulties arising from the cause above described, that it would be an improvement to feed the work to the milling machine in the opposite direction, that is, so the cutting edges of the milling cutter would start in at the large end of the wedge; but whether this would be an advantage (except, perhaps, in cutting shapes) is doubtful. In the roughing cut the cutting edge would be starting in on the scale, and in finishing flat work the cut is so slight that there could be but little gain.

Milling machines were for years thought to be well adapted for

small work, but not for large, and this idea is likely entertained by many yet, while others believe that the reason it has not proven successful in large work was because there were no large machines, while the truth probably is that the kind of work has more to do with the question of adaptability than the size, and that certain classes of work of whatever size can best be done by rotary cutters, while other kinds will always be best done by the planing, shaping and slotting machines.

What cutting action shall be adopted to perform a certain operation is as much a question in machine designing as any other. The most perfect of all machine work is done by an emery or corundum wheel, and the next of all I have seen is done with what is usually understood, though perhaps not properly, an end mill. Cutting tools, traversing in curved paths across a flat surface, have an advantage in never following over twice in exactly the same path, as is the case in a planer tool, which probably accounts for the absence of chatter; but, on the other hand, the cutting blade traverses over eleven inches of distance to finish a seven-inch cut. The planer wastes time in stopping, running back and starting, while the traversing blades are always at it.

After a half century of study over the lathe and planer, improvement still goes on, and in this view what may we not expect a quarter of a century hence of the milling and traversing cutters and emery-cutting devices?

In all our planning and all our study there is one vital principle that is hardly ever thought of and never mentioned. We all look for the best new machine, but who to see which is to be the best old one? The mathematicians tell us which is the most perfect gear, but all their calculations are based on a new one, and no thought given to the one-half or three-fourths worn out. An engine cylinder needs no counter-bore when new, but experience has shown the necessity for it when old; and short cross heads, working on long slides without overlapping at the ends, soon wear the slides concave and themselves rounding, with the result, two curved surfaces of unequal radius always growing worse. The slides I will speak of later on. The gear tooth needs an undercut at the root as much as the cylinder needs a counter-bore. The epicycloidal may be the best form for a new gear, and the single arc odontograph for an old one.

With the exception of the J. Morton Poole grinding machine, nearly every machine tool depends for the accuracy of its work upon the accuracy of sliding surfaces. In the best work special methods are adopted to make them true, after which they are left to fate to keep them so. Who has ever deliberately undertaken to plan and build a planing machine that should maintain perfect truth during its entire existence? The planing machine has a short table working on a long bed, the milling machine a long table working on a short bed—the lathe a short rest on long slides, and the shaper a long slide in a short guide.

The tool builder would laugh at the engine builder who should run a cross head on guides not cut away so the cross head would overrun at the ends; and yet he makes the sliding block on the cross feed of his lathe six inches long working on a dovetail two feet long, sometimes running off at one end and never at the other. The sum of the two fitted surfaces is thirty inches. If divided equally, that is, a fifteen-inch sliding on a fifteen-inch dovetail, the two would remain true and be a good job as long as the lathe lasted. This is one of the places where for lathes of moderate size there is no excuse for anything but wearing surfaces of equal length. The designer who finds any difficulties in this that he cannot overcome, has too little genius for the job. Equal length sliding surfaces can usually be employed in all the slides on milling machines, punching machines and many others, and wherever possible the use of any other arrangement is simply poor designing, to give it the mildest criticism possible. A short sliding block on a long guide invariably gets bad sooner or later, and refitting becomes necessary.

The sliding-head of a shaper invariably made longer than its guide, is an ever-present nuisance and one that can be abated. The requirements call for a slide to work at all sorts of stroke from an inch to a foot and at positions varying as much as a foot. As now made, much of the back end of the sliding arm does not get a 100th part of the wear that the front end does, and often none at all. As a result in many establishments the thing may be worked for a year at short stroke and in one place; then when the attempt is made to use it in a different position, it is so loose in one position and so tight in another as to be worse than useless. The remedy is easy. Make the two sliding surfaces of a length

and attach the connecting rod to an immovable pin, then both ends of the slide will always overrun, whether the stroke is an inch or a foot. To provide for the varying position of cut, the plan shown in *Fig. 3*, is presented.

This slide is guided in the usual way and driven by a connecting rod rigidly attached to the side or bottom, as most convenient. Instead of the slides carrying a head and tool post, it is bored out, split and provided with two binding bolts, so as to grasp the tubular arm, which, when loose, may be slid or turned in any position, and then firmly bound in that position.

The diameter of the hollow projecting arm need not be much larger than the width of the tool holder—five or six inches—in an ordinary ten-inch or twelve-inch machine. The sliding arm being provided with a featherway and surrounded by a graduated worm-gear or ring feathered to it, the bar may be rotated to any angle and adjusted endwise without disturbing or losing the angle at which it is set. A second advantage of this arrangement would be the removal of the ordinary side projections which so frequently come in the way when using the present machines.

I have not presented this so much to represent an improvement in shapers, as to show how it is possible in machines of this class to use slides of equal length, maintain their proper position and get adjustment of tool. But you will say it is not so simple. True. Neither is a watch as simple as an hour-glass, nor a Corliss engine as simple as a Hero. Good things cost more than poor. All improvements have to be paid for. Sliding surfaces, such as those of the planing machine and slide rest of a lathe, cannot be of equal length; then the question arises, are they made to maintain their truth as well as possible?

The table or platen of a planing machine cannot be used successfully for its entire length unless the bed upon which it runs is longer than itself—usually about one-half longer—and when so arranged and used to any considerable extent on short work it wears the bed concave and itself convex to fit. This would occur even were both absolutely rigid, but is much aggravated by an elastic table, which may be sprung one way or another every time a new piece is secured to it. A rigid table with wearing surfaces so generous as to always float on the oil, however heavy the load, would keep true a long time, or if the wearing surfaces were cut away

towards the end of the bed, as shown by this model (see *Fig. 4*), the result would be practically perfect. If the slide rest of a lathe be long enough, strong enough, has wearing surface enough of the right sort, and any man has courage enough to cut away the ways on the plan just shown, one would remain true for a time far beyond anything yet produced. And so with all slides; in fact the principle can be applied to all wearing surfaces where from necessity a short piece must work on a long guide, or a short nut on a long screw.

Journals with end play should have bearings and box of the same length; collars, or the bosses, on wheels that resist end thrust on shafts, should rest upon or run against bosses of exactly their own diameter. The guides or slides of steam engines are as often too long as the cross heads are too short, for one is as fatal to enduring truth as the other.

I have used the best-known tools to illustrate principles that are employed in all machines; the assembling or combining the sliding, the rotary, angular and screw motions, is invention or design in proportion to the extent of originality. The construction of the framing calls for the exercise of many faculties; it must be suitable, strong enough, rigid enough (entirely a different quality), of such form that the patterns can be readily made, if to be of cast metal, so as to be easily forged, if of forging, so it can be machined; and last, but not least, so it will look right—in fact, lacking nothing but the poetry to be artistic. This looks, this artistic, this good or bad, this one thing that is past reason, this thing that we can't argue about, but feel, is that part that much more can be thought about than said; that part which we may never be able to tell what to do to be right, and at most, but few things that may be depended upon as positively wrong.

Whatever nature does she does right; whatever the designer does different from what nature would do if she had the same work in hand, cannot be right. Nature at her best, roots her trees to the ground, tapers their trunks, and stretches out their far-reaching branches, but the branches all combined would not exceed the trunk in size; and the branches all grow out from the trunk on one and the same plan—never two or three growing up and then one down, as we find them in some late drilling machines, but all follow the same grand plan. If there is ever an exception to this

in nature it is by distortion, a deformity, an abortion. There has not yet been so many good machines designed to call for an abortion for the sake of variety, for, in fact, there has been so many abortions that the good ones make the variety.

The causes that have been instrumental in producing such a large percentage of poor designs, were these: our early draftsmen were architectural draftsmen; and when the aspiring mechanic required a drawing for his new machine he employed the architect, who brought with him his ornaments and forgot the principles. His posts were columns without cornice or pediment, and when his frame, which he did not understand, did not look right, he attempted to ornament it into shape. This state of affairs was made worse by another set of draftsmen who neither understood architecture nor mechanics, and lacked genius. Machines made from such designs have been scattered throughout the country, imitated and counterfeited, with their worst features made most prominent; cuts published in all the technical journals without a comment as to their merits or demerits in design; and now, to add to this, our colleges are going on from bad to worse by placing before their students models of motions and machines, constructed as machines never are constructed, or would not work if they were, and placing the students under a professor of drawing who teaches them how to make drawings rather than how to represent a properly designed machine, or teach them to know what constitutes good and bad designing.

The columns of all the best buildings in the world are straight —that is, their axial line. When their own weight is a considerable fraction of the whole weight, they have a considerable taper; when their own weight is but a small fraction, there is only taper enough to make them appear straight. I do not believe a computation was ever made to determine that this was right, but the designers were led to it by their own instinct. When the sides of a tapering column are perfectly straight, they will appear concave. This fact, known to the architects in Balbec days, was remedied by making the lines curved just enough so the columns looked straight. Their imitators, discovering the fact without being able to appreciate it, did as imitators usually do, magnified their discovery and mutilated a design they could not appreciate.

Take the columns of the different orders and styles of architecture—Doric, Tuscan, Ionic, Corinthian, Norman and Gothic. No man at the present day will attempt to add to or take from in the hope of betterment, and yet the axis of every one is a straight line. No one of sense will ever attempt to improve them by making them crooked, because the straight line is right; and so with all the successful single-post machine designs—they are straight. It is true that George Richards, of Manchester, gives the sides of the post a noble sweep, but the axial line of the post remains straight.

It is true the supports of a machine are subject to oblique as well as vertical strains, while the columns of a building are not; but why do we fancy that a crooked member will resist those strains better than a straight one? The reverse curve is a line of beauty—yes, in an ornament, but in a thing, not, unless there is a constructive demand for such a curve. The outline of a Porter-Allen engine bed is a reverse curve. True; and it's also true there was no room for a straight one. The noblest architecture is grand because it is consistent. Our machine designs are noble in proportion to their consistency. Every feature of every noble style of architecture is conceived in the same spirit. Every feature of a Grecian building, its columns, its openings, its mouldings, itself, were in one style, and so of the Roman, the Gothic and the Norman, and every feature of a machine design should possess the same general characteristic. The Grecian and Roman columns were round, and for the service they had to perform round was the best possible form they could be. The Gothic columns are not round usually, because for carrying the arched ribs of the vaulted ceilings, clustered columns conform to the arches and best serve the purpose. In machinery, a square or rectangular form is best, because, usually, there are things to be added, and brackets or slides are most easily attached to a flat surface. The shape is consistent, and will never be improved upon. It is one of the things no Queen Ann fashion can overthrow. Fashion follows in machinery as well as in other things, and when it follows because it is the fashion, it is more likely to follow the bad than the good. All our older mechanics can remember when the posts of an iron planing machine were formed to fit various ogée curves—that so-called line of beauty. Does that form look well to anyone to-day?

It has given way to the bold sweep of a single curve, which has come to stay, not simply because it looks better, but when rightly drawn it is right and looks right.

The true form is not a perfect parabola, however, because the case is not one parallel to the fixed beam loaded at one end. The planer post never gets its strain at the extreme end. We cannot trust to the mass of our mechanics to produce tasty designs, because the spirit of our people is not artistic.

The old form of framing and that used yet by the builders of cheap machinery, is H or U section—a style supposed to be strong because the mathematicians say an H section is a strong one, but it is no stronger in any direction than the box and infinitely weaker when subject to torsion. The patterns are costly to make and easily broken in the foundry; there are no cores to make but lots of mending up to do. Your city is not only blessed with good designers, but with good pattern makers and founders besides. I think if a Philadelphia designer should go elsewhere the first man he would send home for would be his old pattern maker and the next the boss foundryman. One who has never tried it can hardly realize how little is the difference between a graceful curve and an ugly one. The man who fails to discover this difference is the one who just fails, whether as a designer or pattern maker.

If an Englishman were set to improve the German design he would put in more iron; if the Frenchman, he would make the form more graceful; if the native Yankee, he would paint it green and stripe it with red; and to change this spirit, the example set by Philadelphia has done more than any other one thing, and it has not been so much by substituting steel-colored paint for green as the substitution of consistent things and forms for inconsistent ones, and doing the thing that was to be done in the simple, right way. This consistency comes, I believe, more from inherent common sense than education, and doing the thing right is more the result of good judgment than mathematical calculation.

It may not be possible to separate in our minds very completely machine designing from mechanical engineering, but in the sense I am endeavoring to devise rules for the one, I feel sure our colleges are entirely overestimating the importance of the higher

mathematics. When one has to use his judgment whether to give a factor of safety of four or ten, his judgment will tell him about how large to make the thing any way; and in another sense, where what Oberlin Smith calls the "anvil principle" becomes an element, the figures are entirely misleading. Or, take the case of the fly-wheel of an engine. It is impossible to begin to figure at all until you have guessed upon the first factor—that is, what per cent. of variation in speed will you accept? For absolutely uniform speed is an impossibility in an ordinary engine, and the extent of variation admissible is entirely guesswork. If you guess twice too much your wheel will be four times too small, and so you will be twice as likely to get it what you want if you guess at the wheel and leave out the figures entirely. It is true that here and there mathematics are useful. In certain cases the higher mathematics are necessary, but usually arithmetic covers the ground, or a few marks on the drawing board, and the slide rule, is a much quicker way. When a class of forty students spend a good part of four years in learning the higher mathematics, with the certainty that at least thirty-eight of them will never have any use for them, and that the other two will forget a large part before the time for them to put them to use arrives, is it not worth questioning the real value of so much application. "Discipline the mind" may be all very well when the mind is the thing that is to do the work, but where something is to be made the mind should never, in the sketch, the drawing or in the mathematics, lose sight of the *thing*, to worship the methods.

For the designing of buildings, the designers, architects, receive special training; for the designing of machines, with some stingy exceptions at our colleges, no training is had, and every man does it himself.

If anyone is to build a house he employs an architect to make a plan; if we are to build a machine, none of us think of employing a machine designer. I have known one exception to this. A manufacturer bought a patented invention, with nothing to show what the machine was to be but a wood model—crude in every element. He employed a man to get up some drawings of it, and with the instructions to "put it in shape." For the drawings and the putting it in shape he paid $25, and the result has been quite a fair fortune. If manufacturers would employ designers

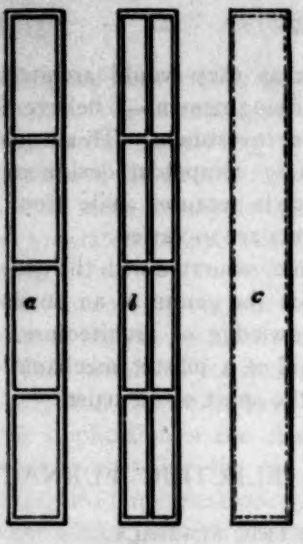


FIG. 1.

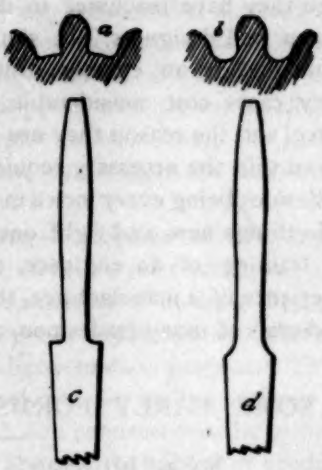


FIG. 2.

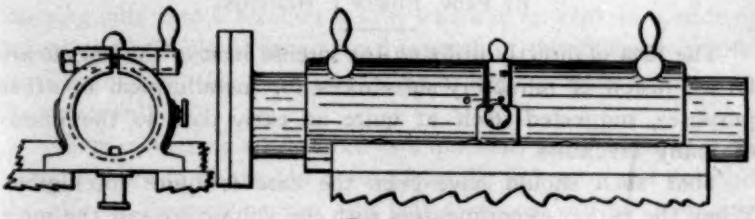

FIG. 3.

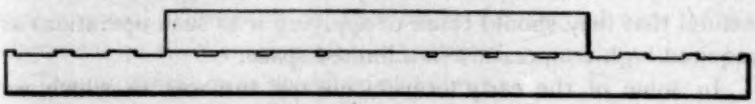

FIG. 4.

*a*, *b*, *c*, *Fig. 1*, illustrate the models shown by Mr. Sweet, which represented three forms of lathe and planer construction. The box form *c*, proved to be fifty per cent. stronger in its vertical direction than either *a* or *b*, fifty times stronger sidewise than *a* and twenty times stronger than *b* and more than thirteen times stronger than either when subject to torsional strain.

*a*, *Fig. 2*, represents an ordinary pinion tooth and *b* shows one of the same size strengthened by cutting out metal at the root; *c* and *d* were models showing the same width of teeth extended to six times the length, showing what would be their character if considered as springs.

*Fig. 3* shows the suggestion in regard to shaper slide, and *Fig. 4* the method of cutting away the ways of a planer so as to reduce the wearing surface of the bed to the same extent as the table. In small machines the suggestion was made to cut out the spaces by milling, and in large surfaces, such as a planing-machine bed, cast out the spaces and fill them with soft metal.

when they have machines to design as they would architects—I mean real designers, not simply draughtsmen—I believe they would find it an equally profitable investment. This would in many cases cost considerable, because competent designers are scarce, and the reason they are scarce is because, aside from the special gift, the necessary requirements are so varied.

Besides being every inch a mechanic, saturated with the courage to do things new, and right, one needs the genius of an inventor, the training of an engineer, a knowledge of architecture, the experience of a manufacturer, the skill of a master mechanic, the handicraft of many tradesmen, and the spirit of an artist.

## On SOME EARLY FORMS of ELECTRIC FURNACES.

### No. 4. JOHNSON'S ELECTRIC FURNACE.

By Prof. Edwin J. Houston.

The idea of directly utilizing the intense heat of the voltaic arc for the fusion of refractory substances, for metallurgical or other purposes, suggested itself at quite an early date to the minds of many inventors.

That such should have been the case is quite intelligible. When the earlier experimenters with the voltaic arc saw the most refractory substances melt readily in its very great heat, it is natural that they should think of applying it to such operations as required high temperature in a limited space.

In some of the early forms of electric furnaces to which we have already referred, the heat of the arc was indirectly utilized.

That which we shall now describe employed this heat directly for the smelting of ores.

On the 22d of March, 1853, John Henry Johnson, filed in the office of the Commissioner of Patents of Great Britain a provisional specification for Letters-Patent for an invention entitled "Smelting Iron and Other Ores." This application was numbered 700 of 1853. The completed specification for the same was never filed.

Johnson describes a furnace in which metallic ores, particularly iron, are smelted in a furnace the heat of which is of electric origin. The ore to be treated, previously ground, was mixed with charcoal

and dropped between the poles of large electrodes across which a voltaic arc was established. The ore thus treated was separated into molten metal and slag by being received in a suitable vessel placed above the electric furnace proper, in which it was maintained in a fused state by any suitable heat. In this vessel the molten mass separated in layers of different densities, in the well-known manner.

The inventor gives the following description of his form of furnace in the provisional specification before referred to.

"This invention consists in the smelting of metallic ores, particularly iron ore, which have been previously mixed with charcoal, by the application of the electric light to that purpose. This is effected by dropping the ore or metal to be melted between the poles of two large electrodes, which are connected in the ordinary manner with a galvanic battery. The electric light thus produced smelts the ore as it passes through it, and the melted metal with the slag falls into a receiver below, where it is kept in a state of fusion by a suitable furnace placed beneath it, the different specific gravities of the 'slag' and melted metal keeping them separate in the receiver."

The specification also describes a modified form of furnace in which the ore to be treated is automatically fed into and through the electric furnace proper. In this form, as will be seen, one of the electrodes is made hollow and contains the mixture of ore and charcoal. Provision is also made for the advance of the electrodes on their consumption in the arc.

This modification is described as follows:

"By another arrangement the two electrodes are placed at a slight angle, and the higher one of the two is made hollow and filled with the ore to be reduced. This ore is gradually pushed forward, as it melts, by a piston and screw rod revolving within the hollow of the electrode. When the whole has been reduced, the piston is withdrawn and the electrode filled again. As the electrodes are consumed, they are caused to advance by screw spindles working into fixed nuts, and attached to armatures or sockets on the end of the electrodes, as by the apparatus a continuous supply of electrodes may be attained in various other ways."

Johnson's electric furnace, differed from any as yet described in the fact that, although the heat employed for the direct fusion

and treatment of the ores was purely electric in its origin, there was combined therewith a second furnace in which the fused material collected and separated into molten metal and slag. That is to say, this early form of furnace consisted of the following elements in combination, viz:

(1) An electric furnace proper, the function of which was to directly fuse and treat the ore or other material by the direct application of the heat of the voltaic arc.

(2) Of a subsidiary or secondary furnace the function of which was to receive the melted and treated materials, and maintain them at the temperature of fusion until a complete separation was effected of the slag and the molten metal as a joint result of the action of such heat, and of their differences of density.

The combination of these two elements in an electric furnace, was, considering the date of the invention, exceedingly ingenious. We see no reason why a furnace constructed on the lines here pointed out should not be practical for the smelting or reduction of certain ores.

It is but proper to remark that the name Johnson appended to the provisional specification, may be that of the agent to whom the same was intrusted and not that of the inventor. No mention, however, is made in the specification, as is usually done, of the same being a communication, and it is possible that Mr. Johnson was the inventor. The disadvantages of thus taking out the invention in the name of the attorney, in place of that of the inventor are manifest, and require no further comment.

CENTRAL HIGH SCHOOL,
PHILADELPHIA, *January 14, 1888.*

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## No. 5. WATSON AND PROSSER'S ELECTRIC FURNACES.

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In the early forms of electric furnaces thus far described, the electric heat has been the sole, or at least the principal source of heat employed. In the forms of furnaces described by J. W. Watson and W. Prosser of England in 1853, the electrical heat formed but an inconsiderable part of the heat employed.

The Watson and Prosser furnaces are described in Brit. Pat. No. 5 of 1853, for "An improved method of manufacturing steel and of carburizing iron."

The electric current is employed for the manufacture of steel in three different ways, viz.:

(1) A mass of molten cast iron is obtained in a suitable furnace or cupola by any of the usual methods. Carbonaceous matter is then added in excess to the metal which combines with it. Two carbon poles, formed of pieces of graphite or gas-retort carbon, are then introduced into the mass, and so connected with a powerful battery as to cause its current to pass through the fused metal. Quoting from the specification of this patent.

"We do not begin to pass the current until the cast iron has been in contact with the carbonaceous matter about two hours. The action of the current is to drive off a portion of the carbon combined with the cast iron, and to reduce the hyper-carburet of iron to a carburet of iron containing 1·5 to 1 per cent. of carbon, or, in other words, to a steel of the composition and properties of shear steel, which has been equalized in its texture and composition by repeated tiltings."

The process just described was also useful, it was claimed, in removing the sulphur or phosphorus with which the metal is so frequently contaminated, and whose presence is so injurious to the steel formed therefrom.

(2) The converting trough in which the bars of iron are submitted to the process of cementation was insulated, and an electric current passed through the bars of iron while packed in the charcoal or other form of carbon. This is described in the specifications as follows:

"We insulate by means of non-conducting substances, such as fire-clay and fire lumps, the ordinary converting trough for making bar iron into steel, and then pass an electric current through the whole number of bars in contact with the carbon or charcoal by uniting their separate ends by stout copper wire of not less than half an inch in diameter clamped on to the ends of the bars, which should be permitted to project through the end stones of the cementing troughs about six inches. By passing an electric current thus through the bars the operation of steeling is much hastened, since bars which by the ordinary process of cementation require from six to eight days to be made into shear steel, may by the passage of the current be steeled in four or five days."

A curious advantage, in addition to the decrease in the time

required is claimed for the electrical cementation of iron. Steel thus formed is, it is claimed, much more uniform in its texture. The steelification is so equalized throughout the mass that the formation of the blisters so common in cemented bars, from the escape of gaseous products, is entirely avoided.

(3) Another process for the formation of steel from iron consists in connecting the mass to be steelified with an electric source by means of poles of graphite or platinum. This is described in the specifications as follows, viz.:

"We carburize iron by making the carbonaceous matter in connection with the metal a conductor of electricity by placing poles, either of graphite or platina, into the cementing substance, such as charcoal, the current then passes equally through the mass. By adopting this mode of carburizing, a steel of moderate hardness is produced in about two days from the commencement of the cementing process. This steel is highly useful for the manufacture of saws and springs."

The broad idea of using electric currents in substantially some of the ways here indicated for the purposes of improving cast iron, or for converting iron into steel, appears to have repeatedly suggested itself to metallurgists, as a few references will show.

Arthur Wall, in British Patent No. 9,946 of 1843, for "Certain Improvements in the Manufacture of Iron," proposes to subject the iron while in the molten state and while congealing to a current of electricity. He also proposes applying electricity to "iron in a smelting furnace or cupola" by inserting one battery terminal at the top hole, and the other at one of the tuyre holes. He suggests the application of electric currents to "the iron in the puddling or balling furnace." This is effected by connecting one battery terminal with part of the fused metal and the other stirring or balling rod.

F. H. Holmes, in describing, in Brit. Pat. No. 573 of 1856, the magnetic-electric machine so well and favorably known in England some time ago, in connection with the name of the inventor, claims the use of the current produced by this machine, as follows:

"In the manufacture of iron and steel for the purposes of obtaining a better product and in less than the ordinary time" and for "The separation of metals from their ores in any of the ways already proposed for applying the agency of electric currents."

W. H. Dawes, in British Patent No. 1,087 of 1867, for "The manufacture of iron," proposes to pass an electric current through molten iron.

E. C. Monckton, in British Patent No. 1,890 of 1869, for the "Manufacture of iron and steel," proposes "To produce iron direct from the smelting furnace in its greatest purity" by the use of electric currents passed through the molten metal.

Chas. Motier Nes, in British Patent No. 866 of 1871, for the "Manufacture of iron and steel, and re-working of old iron and steel," describes the use of electricity in connection with the various processes mentioned.

Forquignon and Ehrmann, in British Patent No. 2,712 of 1872, for the "Purification of cast iron, malleable iron, cast steel, and malleable steel, etc," proposes to employ currents from magneto-electric machines to improve the quality of molten metals.

The fact that none of the processes here described for the utilization of electric currents appear to have been carried into practical operation, does not speak favorably for their efficiency. It does not, however, appear improbable that considerable aid may be afforded to the metallurgist from a slight modification of some of the preceding processes. Such advantages would seem to be indicated in the following general direction, viz.:

(1) The direct formation of certain alloys by making the constituent ingredients thereof the terminals of a sufficiently powerful electric source, and combining them directly through the agency of the voltaic arc taken between them.

(2) The more intimate union of the constituent ingredients of alloys by passing electric currents through them while in the melted state by dipping therein inert electrodes.

(3) The purification of metals by a species of electrolytic separation, by passing electric currents through them while in a melted condition by dipping therein electrodes capable of uniting with the electro-negative, or electro-positive impurities, or with both.

That some of the processes as described, without any modification whatever, may have proved of a considerable value, would appear to be quite probable.

CENTRAL HIGH SCHOOL,
PHILADELPHIA, *January 21, 1888.*

# JETTIES FOR IMPROVING ESTUARIES.

By Lewis M. Haupt, A.M., C.E., Professor of Civil Engineering, University of Pennsylvania.

For centuries the efforts of engineers have been directed towards reducing the heights of the bars which obstruct the entrances to harbors or rivers on alluvial coasts.

They have attempted to apply the concentrated ebb stream to a limited portion of the bar by means of jetties reaching, at times, above high water, in other cases to half tide, and again being entirely or partially submerged. As the resources of the profession appear to be limited to such constructions, aided by dredging, and as the practice seems to be to follow the precedents furnished by experience, it will be instructive to note the results of these structures upon the entrances where deeper water is desired.

In considering the results produced it will be found convenient to classify the structures, according to their conditions of exposure, into three groups:

(1) Where there are tidal fluctuations of considerable magnitude, with but small interior tidal reservoirs and little or no fresh water drainage.

(2) Where the tidal oscillations are small and the land discharge large; and

(3) Where there is a considerable fresh water volume débouching into a tideless sea or lake.

In the first class, where the attempt has been made to improve the channel by scour between jetties, the works have in general proven unsuccessful, as they have invariably resulted in advancing the general shore line, and in pushing the bar further to sea without material increase of depth over its crest. This inherent defect has been partially overcome by constructing large sluicing reservoirs contiguous to the channel and by the frequent recourse to dredging.

In justification of such works for tidal estuaries in this country, frequent reference has been made to the success of the jetties at

the Sulina mouth of the Danube; to the various harbors on our great lakes; to Swinemunde, on the Baltic, and to the South Pass, on the Gulf of Mexico, but the instances are unfortunate for the reason that the seas are tideless or nearly so, and the volume of fresh water discharge is very large as compared with the tidal prism, thus placing them all in the second or third group.

The only case in this class that has come under our observation which has approximated to a successful result, is that at the mouth of the Liffey, at Dublin Harbor, where the improvement is largely due to a gap of 600 feet left in the north wall at its shore end through which the ingress of the flood tide is facilitated. To this extent it confirms the theory and plans which are suggested herein for the difficulties encountered in such cases.

Extracts from a paper read May 20, 1879, by Jno. Purser Griffith, Assoc. M. Inst., C.E., on "The Improvement of the Bar of Dublin Harbor by Artificial Scour:"

"Mr. Giles recommended that an opening should be left at the shore end of the proposed wall 600 feet wide, to allow of a free passage for the tidal waters north of the Green or Bull Island, as the sand island on the North Bull was called. This opening had been proposed by Capt. Corneille in 1802; but at that time the Directors General of Inland Navigation feared that the tide flowing through this opening would carry sand from the North Bull into the harbor, and that the injury thus done to the port might more than counterbalance any advantage to be gained by the opening.

"In 1835, Sir Wm. Cubitt reported upon the state of the harbor. Referring to the improvement of the bar, he says: 'The great increase of depth and improved channel over the bar, I attribute entirely to the erection of the Great North Wall, a measure founded upon sound principles and carried into effect in a manner well calculated to effect the desired purposes, viz., that of checking the influx of sand upon the flood tide from the North Bull into the harbor and giving an increased impetus at the ebb tide by means of narrowing the stream, and confining it in a direction suitable for keeping open the best channel, the effect of which is already shown by an increased depth of five feet over the east bar since the erection of the Great North Wall.'"

Capt. Washington, in his report, written in 1845, on the harbor of Dublin, as one of the Tidal Harbor Commissioners, referred to the Great North Wall as follows:

"The propriety of this measure, which involved so heavy an expenditure, has been a subject much controverted amongst persons connected with the port, but it is believed that there is now but one opinion as to its beneficial effects, and that the plan evinced both good judgment and skill."

There are few, if any, cases of a bar and entrance channel to any harbor

being increased in depth like that of Dublin, viz.: about seven feet in thirty years, and great credit is due those who designed as well as those who executed works which have achieved so important a result.

The improvement of Dublin Harbor entrance ranks second to none. There is no other example, so far as the author is aware, of the construction of an artificial estuary for scouring purposes which has proved so successful.

DISCUSSION.

Mr. Abernethy, Vice-President, said he thought attention should be directed to the construction of works which would tend to increase the tidal volume, and at the same time to prolong the action of the outgoing currents from the period of half ebb towards low water.

Mr. Bergeron stated that it was very difficult, almost impossible, to dredge sand bars in an open sea.

Mr. Vernon-Harcourt, that to diminish the tidal capacity within a harbor was the worst thing that could possibly be done.

Mr. Griffith said: "Prior to the construction of the Great North Wall the ebb of a spring tide attained a velocity of one and one-fourth miles per hour across the bar, while at present it reached nearly three and one-fourth miles, showing an increase of about two miles per hour." He did not believe that very high velocities were efficient. Several instances might be named in which high velocities were attained, and yet the scour was a failure.

Mr. Stoney thought that though there might be some doubt as to the authorship of the Great North Wall, there could be none as to its complete success.

In the application of the jetty system to the alluvial harbors of the Atlantic and Gulf Coast, an effort has been made to compromise the conflicting requirements of *free ingress to flood* and concentration of ebb by building submerged structures of brush mattresses, with a rip-rap covering. These jetties subsided rapidly, and in some instances shrank over fifty per cent. of their original volume, making them cost more than stone. Their effect in an incomplete state has been to push the bar seaward and to induce a scour along their edges, requiring protection by spur dykes without at the same time materially increasing the depths over the bar.

For the ports on the Gulf, the question is one of vital importance. The state of Texas alone embraces 237,504 square miles, yet the best harbor, which is found at Galveston, has but thirteen feet of water and a mean rise of tide of only 1·1 foot. Whatever plan gives greatest promise of a successful issue should be the one applied, and it must be based upon a knowledge of the resultant forces operating at that entrance as revealed by a comparative study of its physical hydrography.

Without giving a critical, detailed statement of these conditions, a succinct history of the changes will indicate the main points to be considered in designing a structure applicable to this site.

HISTORY.

The U. S. Coast Survey of 1867 revealed a remarkable progression of the shore-line and islands to the southwestward and a shoaling of over three feet on the inner bar. To resist these changes, the U. S. Engineers began the collection of data by surveys in 1868, and upon these have submitted several plans for accomplishing the desired object. McAllister's hope was in dredging; Howell's in parallel jetties of gabions, which are now beneath the sands; Mansfield and the Boards of Engineers trusted to submerged jetties, one of which was partially built; Eads believed that high convergent jetties would produce the required result, and in this view the permanent Board of Engineers has at length acquiesced as the only proposed plan that has not been tried. So a score of years have passed and there is virtually no better water, yet in the Report of January 21, 1886, the Board of Engineers says: "Deeper water on the bar is needed, and the question to be considered is, how that deep water can be obtained. The methods are two: (1) by dredging alone; (2) by using tidal scour between jetties, aided, if necessary, by jetties. As to the first method, it has already been tried unsuccessfully." * * * "The second method would include the obtaining of a deep channel way to be protected by jetties from the deteriorating effect of wave action, and from the influx of sand, and the maintenance of that channel way by the aid of tidal scour, supplemented, if found necessary, by dredging. The jetties should be so placed as to secure the greatest tidal scour practicable, without seriously injuring the interior harbor, and without greatly endangering the safety of the jetties against undermining or of Galveston Island from overflow in great storms. The greatest scouring effect will be obtained, and the greatest security against undermining, by making the jetties tight and by raising them above high water." * * * "Cost of jetties complete to thirty-foot curve, $7,000,000, their aggregate lengths being nearly 54,000 feet. This estimate supposes that the money is freely supplied."

UNCERTAINTY AS TO RESULTS.

When so large an amount is involved (although it is small as a measure of the interests at stake) we should expect to find a reasonable amount of confidence as to the results to be obtained. It will be seen that the project is dependent upon tidal scour, and that this can only be obtained by concentration of the *ebb* currents upon some point of the bar. To effect such concentration the flood tide must be freely admitted to the inner bay, so it may be "filled at every influx of the tidal wave," for if the flood is diminished in volume, the ebb will be likewise reduced, and if a part, say one-half, of the gorge between Fort Point and Bolivar Island were filled up, it would restrict the flood in nearly the same ratio. High jetties would produce such an effect by cutting off the incoming tide from the funnel-shaped entrance of the gorge, and practically transferring the latter to the crest of the bar where the velocities are much less. That such a result is anticipated by the Board appears from their report just referred to, where in discussing the

WIDTH BETWEEN JETTIES,

they say, "Such a jettied channel offers more resistance to inflow than does the present entrance; reduces the present tidal prism about one-third; allows the bay to fill more slowly than the present entrance does, and hence gives greater differences of level than that given by the present entrance;" and they add: "Diminishing the interval between the jetties from 7,000 to 3,500 feet, changes the difference of level between the bay and gulf but slightly. Through the opening of the same depth and double width, about twice as much water will flow in and out, but as it flows through a double cross-section its velocity is nearly the same, * * and the depth will be nearly the same." Again, on page 18, the Board says, "the jetties will diminish the freedom of inflow at Galveston."

Another Board, composed in part of the same officers, reported with reference to the jetties at Sabine Pass, in these words: "Now if jetties are built from the shore, 1,800 feet apart, as proposed, across the bar to deep water, the pass is virtually prolonged three and one-half miles, and its mouth transferred to the same distance seaward. The funnel-shape of the entrance way will be lost, and the surface slope of the channel of this elongated pass will be diminished, both of which changes will decrease the tidal flow."

From these extracts it would appear that the authorities are agreed that concentration of ebb for tidal scour can only be obtained at the expense of the flood volume, and that the successful application of jetties depends upon a proper adjustment of the area and length of the channel between the jetties to the tidal volume of the inner bay.

GREAT WIDTH BETWEEN JETTIES.

Upon this point Capt. Jas. B. Eads says: "The slope (producing scour) results from the difference of level between the surfaces of the gulf and the bay. Any one can see that the more freely the water can flow from one to the other, the less difference will there be between the heights of their respective surfaces, and consequently the less will be the slope of surface through the channel between them, and the less will be the current velocity. This is certainly the very opposite of what we *must have* to produce deepening. Hence, it is evident that *large openings* and a freer communication with the bay through them, and over low, submerged jetties, are precisely what we do not want."

Hence, while the doctors disagree, the one condemning narrow, the other wide passes, we may leave them to their opinions and read the lessons of experience from

THE BOOK OF NATURE.

When large inner bodies of water are connected with the ocean by small inlets, it becomes impossible for the inflowing tide to fill them to the level of the ocean during the time of the rise or fall.

A small volume may pass in when the outer level is above the inner, and *vice versa*, but the general level of the water in the bay or sound will not be sensibly affected, and it will stand at about the height of mean tide. The flood will then begin to flow in at half tide, and will have its maximum velocity at "high-water-stand," and the ebb will occur between mid-tide and low-water, but as the inlet is small and the inner prism large, the currents will be too feeble to produce marked effects. This action is very perceptible at Albemarle and Pamlico Sounds, where, according to the United States Coast Survey Report of 1862, p. 45, it is said, "the basin being nearly tideless, has its surface lying at about the mean level of the sea—that is, at the half-tide plane—and it is upon this plane that the currents of the inlet must take the initiative. Moreover,

the maximum velocities must occur when the greatest contrasts of height between the ocean and the sound obtain; that is, at *high water* for *flood* and at *low water* for the *ebb*." The more the inlet is contracted, therefore, the less will be the tidal oscillation upon which the inner bay depends for its currents to maintain its channels. *Every construction on the bar becomes more or less of an obstruction to the tidal ingress*, and, consequently, neither high jetties nor submerged jetties will satisfy the conditions.

THE PROBLEM

is to discover a form and position of structure which will not seriously oppose the flood ingress, but which will utilize it for scouring over the bar; will cause it to deposit its load of sand outside the crest and not carry it into the channel; will concentrate a large portion of the ebb for effective work on the bar; will furnish every possible facility to vessels entering the port in all stages of weather, and prove at the same time an aid to navigation.

A solution which appears to fulfil all these fundamental conditions is herewith submitted by the author. It is based upon the fact, which is so frequently observed and referred to by the engineers in charge of improvements along the Gulf coast, that the resultant direction of all the forces acting year in and year out, is to the west and southwest, or, in other words, our coast line drift is rolling in that direction; and upon the further fact that the flood is the potent agent which lifts up the bars in front of our entrances, which the superficial and enfeebled ebb is unable to cut down. For example, the report on Pass Cavallo says, "its history, like that of other entrances on this coast, showed a steady deterioration as a harbor and a constant shifting of the channel to the south and west. Yet the jetty is built on the *south* side of the channel, where it will catch the sand and drop it in the channel and fill it up as surely as a snow fence placed to the leeward of a cut will aid in filling the cut. It thus requires more work of the ebb, which must carry this sand out to the gulf beyond the end of the jetty, and thus extends the bar rapidly in that direction. Estimate for this pass, $1,039,280.

For Arkansas Pass the report says: "And its history, like other entrances on this coast, shows a constant movement of its channel to the south, with progressive shoaling therein and enlargement of

the Gulf Bar." Estimate $1,200,722.75. And the *south* jetty is again built to catch sand and obstruct the natural channel. Again at Brazos, where the estimated improvements were to cost $328,000, "approval was given to so much of the project as covered the construction of the *south* jetty," although "the harbor within was obstructed directly across the mouth of the pass by a bar in the *usual curved form*," etc.* At Sabine Pass, estimated to cost $3,177,606.50, the *west* jetty was built first and made a shoal on its east side, and in 1885 the crest of the bar had been moved seaward 1,900 feet, and it is said, "It is perhaps impossible to decide as to the amount of influence the work done has had upon the depth of water across the bar." And so at Galveston, the same experience is repeated and the same comments may be made as to effects produced.

### WHAT IS TO BE DONE?

Evidently to barricade the sand by a break-water of a concave, curved form, so as to decompose the flood and precipitate its burden of sand outside the channel, and having a straight inshore flank to aid the flood in cutting a beach channel, short, deep and direct between the bar and the shore, not by parallel or convergent jetties, but by a *single line of works having a total length of less than half that proposed for the high jetties, costing less than half the amount and producing much greater effects* in removing these serious obstructions to commerce. Thus nature will be aided in her efforts to build up an island or middle ground with a channel on either hand and a lee-way for all conditions of storm or tide.

* The engineer in charge has recently reported with reference to this entrance that, "The work already done has disappeared without having any effect on the bar."

# ISOLATION OF FLUORINE.*

BY A. E. TUTTON.

One of the most difficult problems of modern chemistry has at last been solved satisfactorily. After three years of incessant labor, occasionally interrupted by temporary feelings akin to despair, M. Henri Moissan has at length isolated in considerable quantities that most baffling of elements—fluorine, and has been enabled to determine its principal properties. The experiments themselves are among the most interesting ever performed, and their details, as described by M. Moissan in the December number of the *Annales de Chimie et de Physique*, form the most fascinating reading. They must of necessity have been extremely costly, for by far the greater portion of the apparatus employed was constructed of platinum, and it is not often that one hears of a platinum tube 80 centimetres long and of 1½ centimetres diameter being destroyed in each experiment, as happened in the earlier stages of these researches.

The isolation of fluorine has formed a worthy object of the attention of chemists ever since the first remarkable experiments of Sir Humphrey Davy, who was rendered dangerously ill by being exposed to the corrosive fumes of hydrofluoric acid. Although Davy was not successful in obtaining free fluorine, yet he brought clearly to light the nature of hydrofluoric acid, and proved it to consist of hydrogen combined with an unknown but extremely active element—fluorine. The history of all the attempts which have since been made to effect the preparation of free fluorine might occupy a volume, and it will therefore only be necessary to refer to the later work of our countryman, Gore, who, in 1869, published his researches upon the electrolysis of hydrofluoric acid, and of certain fluorides, and left our knowledge of the acid itself in a most complete state. M. Moissan, working in the laboratory of M. Debray, now steps in and achieves the result so ardently sought after during the last eighty years—another example of the irresistible power of human perseverance.

* London *Nature*, **37**, 179.

In the light of the experience gained by former experimenters, it appeared that the action of a powerful electric current upon the compounds of fluorine with the non-metallic elements, such as hydrogen, phosphorus and arsenic, would be most likely to yield the desired result; knowing also that fluorine must be an extremely energetic substance, it was absolutely essential to work at very low temperatures. Hence M. Moissan's first attack was made upon the fluorides of phosphorus and arsenic, but finding these to be practically impregnable, he diverted his attack, guided by certain indications afforded during his first attempt, upon hydrofluoric acid itself. Finding, however, that pure hydrofluoric acid is an exceptionally bad conductor of electricity, as has been stated by other workers—that even a current from fifty Bunsen cells would not pass through the liquid—he eventually, after several essays, succeeded in converting it into a conductor by dissolving in it a quantity of the double fluoride of potassium and hydrogen. On passing the current from twenty Bunsen cells through the now conducting medium, hydrogen immediately commenced to be evolved at the negative terminal, while fluorine was with similar rapidity evolved at the positive pole, and exhibited its tremendous activity upon everything that came near it: burning up hard crystalline silicon like tinder, setting fire to organic matter, and forming fluorides with incandescence with many other elements.

Having thus indicated the general course of these researches, it will no doubt be interesting to follow M. Moissan during the carrying out of his principal experiments.

The first series consisted in examining the action of electric induction sparks upon the gaseous fluorides of silicon, phosphorus, and arsenic. The gases were introduced into glass eudiometer tubes standing over mercury, and the spark was passed between two platinum wires connected with an induction-coil actuated by a few Grenet or Bunsen cells. On introducing dry silicon tetrafluoride, $SiF_4$, and passing sparks for an hour, no decomposition was effected, the result being discouragingly *nil*. Dry phosphorus trifluoride, $PF_3$, however, behaved quite differently, phosphorus being deposited upon the inner wall of the tube; but the fluorine liberated at once combined with the residual trifluoride to form the more stable pentafluoride, $PF_5$. Some time ago this penta-

fluoride of phosphorus was prepared by Prof. Thorpe, who also submitted it to the action of the induction spark, unfortunately without effecting any decomposition. Precisely the same result has been arrived at by M. Moissan, using a 0·04m. spark; but on obtaining sparks 0·2m. long, a rapid etching of the walls of the glass tube occurred, and the meniscus of mercury entirely lost its brilliancy. After an hour's duration the experiment was concluded, and the apparatus allowed to cool, when it was noticed that the volume had diminished; moreover, the gas was found to have changed its properties, yielding a precipitate of silica in contact with water, while the residual gas consisted of the trifluoride of phosphorus. Hence $PF_5 = PF_3 + F_2$, which latter forms, with the glass, silicon tetrafluoride, and, with the mercury, fluoride of mercury. So here again the experiment was disappointing, and although fluorine was for the moment liberated, this method was certainly not suitable for the preparation of free fluorine.

Fluoride of arsenic, $AsF_3$, the next fluoride experimented upon, was first prepared by M. Dumas, who was severely injured in the experiment. It is a liquid which boils at 63° C., and may easily be maintained in a gaseous condition by use of a steam jacket, and submitted to the action of the spark. It is, however, a most disagreeable substance to work with, as it produces most terrible sores when, by any mischance, it comes in contact with the operator's skin. On passing sparks through it for an hour, as in case of the pentafluoride of phosphorus, the platinum wires became covered with a black incrustation of arsenic, while the walls of the tube were strongly corroded. On testing the gas, it was found to contain a large quantity of silicon tetrafluoride mixed with a smaller quantity of free fluorine, which displaced sufficient iodine from a solution of potassium iodide to give a good coloration to several cubic centimetres of chloroform. Clearly, progress was being slowly made, though still far from the isolation of fluorine.

And now a remarkable experiment of a new type was performed. It had been noticed that, on passing an electric current through a platinum wire in an atmosphere of phosphorus trifluoride, the platinum fused owing to the formation of a fusible phosphide of platinum; at the same time the glass of the containing vessel was etched and the mercury attacked. So the experiment was repeated on a grander scale. A quantity of spongy platinum, pre-

viously washed with hydrofluoric acid and calcined, was placed in a platinum tube 80 cm. long and of 1·5 cm. diameter; that portion of the platinum tube which required to be heated was incased in a second outer tube of glazed porcelain, so that between the two a current of nitrogen could be kept circulating, and so prevent access of furnace gases. The tube was then heated in a furnace, and pure hydrogen passed through it for some time to remove all other gases; afterwards pure nitrogen was substituted, and finally phosphorus trifluoride. After passing a short time, the current of fluoride was suddenly stopped with a most singular result: a partial vacuum was caused, owing to absorption by the platinum.

When, however, the current of trifluoride was passed more rapidly, a small quantity of pentafluoride was formed; the fluorine liberated, when the absorption of phosphorus by the platinum occurred, having combined with the trifluoride just as in the spark experiment. But, on examining the gas which passed out of the tube under these conditions, it was found to liberate iodine from potassium iodide, attack mercury, and etch glass. In fact, it was proved that free fluorine was liberated, and mostly absorbed by the platinum, causing the diminution of pressure on stopping the current, but being more or less carried away when the current was more rapid. The fluophosphide of platinum formed was found to contain only seventy to eighty per cent. of platinum, and the formation of this substance was so rapidly effected that every experiment required a new tube. The action of pentafluoride of phosphorus upon platinum was next tried, and with still more encouraging results. On sweeping the tube, heated in a coke blast furnace, with a rapid stream of the pentafluoride for some minutes, then moderating the rapidity, and five minutes later again increasing the speed, the issuing gas was found to blacken solid potassium iodide by liberating free iodine, inflame phosphorus, and attack crystalline silicon, glass and mercury. It was, in fact, free fluorine drowned in excess of trifluoride of phosphorus. This was a decided advance, and the outlook was becoming considerably more hopeful.

The next experiments were made with liquid fluoride of arsenic, $AsF_5$, a quantity of which was placed in a platinum crucible, which served as the negative electrode. A platinum wire, dipping into the liquid in the crucible, and reaching to within five millimetres

of the base, served as the positive electrode. The current from three Grenet cells was then passed through the liquid, causing a deposition of arsenic upon the interior surface of the crucible, but no gas could be perceived at the positive pole. However, on dipping the platinum wire into a solution of starch paste and potassium iodide, blue striæ were at once formed in the solution, showing the presence of a condensed gas sheath of fluorine around the platinum wire. Following up this indication, the current from twenty-five Bunsen cells arranged in series was next employed, and immediately the deposition of arsenic commenced upon the walls of the crucible, while bubbles of gas were evolved around the platinum wire. Unfortunately the action soon ceased, owing to the bad conductivity of the liquid and of the thick deposit of arsenic. The wire, however, was strongly attacked. So attempts were next made to increase the conductivity of the fluoride by the addition of metallic fluorides, and it was soon discovered that the best results were obtained by use of the double fluoride of hydrogen and potassium, HF . KF. It was probably this discovery which led to the grand success with which these efforts were finally crowned, for, as has been previously mentioned, it was by the electrolysis of this double fluoride that M. Moissan eventually succeeded in preparing free fluorine.

Before leaving the experiments upon arsenic fluoride, it may be mentioned that it was eventually electrolyzed in a continuous manner by use of seventy to ninety Bunsen cells, the arsenic liberated remaining in suspension in the liquid, instead of adhering to the tube, but the bubbles were rapidly seen to diminish in size in passing through the liquid, and scarcely a trace of gas escaped; instead of permitting its isolation, the flourine preferred to form a new fluoride, the pentafluoride of arsenic, thus once more baffling the ingenious experimenter.

But success was now not far away. The wonderful manner in which the double fluoride of potassium and hydrogen increased the conductivity of arsenic fluoride determined M. Moissan in employing it for the same purpose in an attempt to electrolyze pure anhydrous hydrofluoric acid. Faraday long ago showed that the electric current will not pass through the anhydrous acid, and Gore more recently came to the same conclusion. The current from fifty Bunsen cells was found by M. Moissan to be absolutely

powerless to penetrate the acid used in these later experiments. But, on dissolving a few fragments of the double fluoride, HF. KF, in the acid, the current at once passed freely, and the experiment thus became possible. The apparatus used in the first attempts with this mixture consisted of a platinum U-tube, of which each branch was closed by a paraffined cork, through which the rods of platinum forming the poles were passed. Upon each branch, just above the level of the liquid and beneath the cork, was soldered a little platinum delivery-tube to lead off the gases evolved. As hydrofluoric acid boils at 19°·4 C., the apparatus was immersed in a bath of methyl chloride, which boils at — 23°, but which could be reduced in temperature to — 50° by driving through it a current of dry air. Hence the electrolysis could be conducted without fear of the gaseous products being drowned in excess of vapor of hydrofluoric acid, and the activity of the liberated fluorine was at the same time moderated. On passing the current, a gas was at once produced at each electrode, a regular evolution of hydrogen at the negative pole, and a continuous disengagement of gas at the positive pole. But still affairs were not satisfactory: crystalline silicon did not take fire when held in the gas coming off from the positive pole; so the apparatus was taken to pieces an hour later, in order, if possible, to find a clue to the source of failure. The paraffined cork at the negative branch was intact, but, behold the mischief, the other was carbonized to the depth of a centimetre; so the liberated fluorine had extracted hydrogen out of the cork, and passed on as hydrofluoric acid. The positive platinum rod was also much corroded. Closely-fitting stoppers of fluor-spar were next tried, coated with melted gutta-percha, but the latter again soon melted on passing the current, and was put *hors de service*. Gum-lac and many other substances were tried, but all to no purpose, and much precious time was lost. Finally, however, the difficulty was overcome by using stoppers of fluor-spar, carefully inserted in hollow cylinders of platinum carrying fine screw threads upon their outer surfaces, which engaged with corresponding threads upon the interior surfaces of the two branches of the U-tube. The platinum rods passed through the axis of each cylinder of fluor-spar: the rods themselves were of square section, of 2 millimetres side and 12 centimetres long, and passed to 3 millimetres from the base of the U-tube; they were made of irido

platinum, containing ten per cent. iridium, which is less attackable than pure platinum. The U-tube consisted simply of a platinum tube, bent twice at right angles, 1·5 centimetres diameter and 9·5 centimetres high, and was fitted with side tubes and immersed in methyl chloride as before.

The pure anhydrous hydrofluoric acid, which was the next necessity, was prepared in the following manner. A known volume of commercial acid was treated with sufficient potassium carbonate to neutralize about a quarter of it, and then distilled in a leaden retort over an oil bath at 120°. At this temperature the fluosilicate of potassium, formed from the hydrofluosilicic acid, contained as impurity in the commercial acid, was not decomposed, and the distillate was therefore free from silica. This distillate was then divided into two parts, and one-half, saturated with pure potassium carbonate, forming neutral potassium fluoride, was then added to the other half, and transformed into HF . KF. The double fluoride was then dried at 100°, and afterwards kept for some days in the vacuous receiver of an air pump, containing also strong sulphuric acid and a few sticks of fused potash. When absolutely dry it fell to powder, and was then ready for the preparation of hydrofluoric acid, which was always freshly prepared immediately before each experiment. The dry fluoride was in each case introduced into a recently ignited platinum retort, and maintained at a moderate heat for some time so as to commence the decomposition slowly; the first portions of distillate were rejected, as they would contain the last traces of water. The platinum receiver was then adapted and surrounded by ice and salt; on heating the retort more strongly, pure hydrofluoric acid condensed in the receiver as a limpid liquid boiling at 19°·4, very hygroscopic and fuming in the air.

While the preparation of the acid was in progress, the U-tube and electrodes were drying at 120°. From six to seven grammes of the dry double fluoride were now introduced into the apparatus, the stoppers were screwed in and covered with gum-lac. The whole was then fixed in the methyl-chloride bath, and, until the introduction of the acid, the delivery tubes were connected with desiccators containing fused potash. A constant supply of methyl chloride at — 23° was maintained in the outer cylinder, as a slight rise of temperature allowed of the volatilization of some of the acid.

About fifteen to sixteen grammes of the anhydrous hydrofluoric acid were then gently aspirated into the apparatus, and the current from twenty Bunsen cells allowed to pass, when immediately a regular evolution of gas occurred at each pole. At the negative pole pure hydrogen was evolved, which burnt with its characteristic flame, forming water. At the positive pole was liberated a colorless gas of penetrating and very disagreeable odor, somewhat resembling that of hypochlorous acid, and rapidly irritating the mucous membranes of the throat and eyes. It was no other than pure fluorine itself. All the trouble, all the expense, and all the disappointments were repaid. It must, indeed, have been a supreme moment for M. Moissan.

In order to study its action upon solids, they were placed in small glass tubes, and brought near to the orifice of the platinum delivery tube at the positive side. The test was generally repeated, holding the solids in small platinum capsules.

Sulphur, brought thus near the orifice, at once melted and inflamed; selenium behaved in like manner; as did also tellurium, with incandescence, forming fumes and becoming coated with a solid fluoride.

Phosphorus at once took fire, forming tri-, penta-, and oxyfluorides. Powdered arsenic and antimony combined with incandescence, the former yielding drops of $AsF_3$.

A fragment of iodine placed in the gas combined with production of a pale-blue flame; in an atmosphere of iodine vapor fluorine itself burnt with a similar flame. Vapor of bromine lost its color and the combination was sometimes accompanied by detonation.

Cold crystalline silicon at once became incandescent, and burnt with great brilliancy, sometimes with scintillations. On closing the little tubes containing it with the thumb and opening under water, the silicon tetrafluoride formed was absorbed and decomposed with precipitation of silica. Any undecomposed silicon was found to have been fused.

Debray's adamantine boron also burnt in the gas, becoming incandescent and giving off fumes.

Fluorine has a most extreme affinity for hydrogen; they combine in the dark with explosion. In one of the experiments the electrolysis was allowed to continue several hours, so that eventually the small quantity of undecomposed acid remaining in the

U-tube was insufficient to keep the two gases apart; the experimenters were consequently suddenly startled by a violent detonation. The hydrogen and fluorine had combined in the dark at the low temperature of — 23°. The same detonation was afterwards brought about on a smaller scale by reversing the current. On bringing the wide-mouthed delivery tube of a hydrogen generator near the orifice, the detonation at once occurred, and the hydrogen inflamed.

Metals are all attacked with more or less energy by fluorine, forming fluorides. Cold sodium and potassium were at once rendered incandescent. Calcium, magnesium and aluminium acted similarly, in a more modified manner, becoming incandescent when slightly warmed. Powdered iron and manganese, on gently warming, burned with bright scintillations; lead was attacked in the cold, and tin at a slightly elevated temperature. Mercury, as suspected, entirely absorbed the gas, forming yellow protofluoride. Silver at a gentle heat became coated with a beautiful satin-like fluoride, soluble, unlike the chloride, in water. Gold and platinum at 300°–400° became coated with their respective fluorides, which were decomposed again at a red heat, with evolution of free fluorine.

Perhaps the strongest evidence of the intense chemical activity of fluorine is exhibited in its action upon cold potassium chloride: the chlorine was at once expelled, filling the air with its disagreeable odor, and was identified by the usual chemical tests. Chlorine was also expelled from its combination with carbon in carbon tetrachloride.

All organic compounds are violently attacked by fluorine; a piece of cork at once carbonized and inflamed; alcohol, ether, benzine and turpentine took fire immediately in contact with it.

Glass, as might have been expected, is at once corroded by fluorine; some very delicate experiments were carried out with perfectly dried glass, with the same result.

Many other reactions, all interesting and all showing the immense energy with which the atoms of fluorine are endowed, were performed, but one especially ought to be noticed, viz., the action of fluorine upon water. It is a singular fact that, whenever oxygen is liberated in the cold, there is a great tendency to form ozone: hence when fluorine is attempted to be collected over water, the gas collected is not fluorine, but ozonized oxygen; water is decomposed

by the fluorine forming hydrofluoric acid, while the oxygen is set free, and a considerable quantity of it is converted into the more condensed form of ozone.

On taking the apparatus to pieces after each experiment, the hydrofluoric acid remaining was found to contain a small quantity of platinum fluoride in solution, and a black mud consisting of a mixture of iridium and platinum in suspension. The negative electrode was not attacked, but the platinum rod forming the positive pole was eaten away to a point, so that one rod only served for two experiments. The average delivery of gas was about 1·5 to 2 litres per hour.

With regard to the chemical processes involved in the electrolysis, it appears probable that potassium fluoride is first decomposed into fluorine, which is evolved at the positive pole, and potassium, which decomposes hydrofluoric acid, liberating its equivalent of hydrogen at the negative pole, and re-forming potassium fluoride, which may again be electrolyzed. Hence a small quantity of the double fluoride can serve for the decomposition of a comparatively large amount of hydrofluoric acid.

The double fluoride HF. KF is very soluble in hydrofluoric acid, forming a crystallizable compound, richer in hydrofluoric acid than HF. KF, and which gives off no acid vapor at the boiling point of the anhydrous acid, 19°·4. It is this compound which one ought to seek to obtain for electrolysis, as it is very soluble in excess of acid, forming a liquid of good conductivity.

The double fluoride, HF . KF, itself, was finally electrolyzed by M. Moissan. It fuses at 140° to a colorless liquid which is quite suitable for electrolysis. The experiment was performed, as before, in the platinum U-tube, and, on passing the current, fluorine was again liberated at the positive pole, and at once set fire to crystalline silicon; but the platinum was strongly attacked, so the experiment was stopped in order to save the tube. On plunging a couple of platinum wires connected with the battery into a quantity of the fused double fluoride contained in a platinum crucible, gas was evolved in abundance at each pole, and on bringing the wires in contact, even in the dark, detonation occurred, owing to the combination of the evolved hydrogen and fluorine. At the same time the platinum wire from which the fluorine was evolved was almost entirely eaten away.

In concluding these remarkable researches, which have happily terminated so successfully, M. Moissan discusses very fully the question of the identity of the gas liberated at the positive pole with the element fluorine; and there can be no doubt that he has completely proved this identity, at the same time showing that fluorine occupies the place of honor as the most intensely active chemical element with which we are at present acquainted, and that it assumes its rightful position, theoretically destined for it, at the head of the group of halogens.

## The JAPANESE MIRROR.—II.

F. E. Ives.

(*The Substance of Remarks at the Meeting of the* Institute, *held Wednesday, February 15, 1888.*)

A few days ago I discovered, in a pawnbroker's window, a Japanese mirror, which is interesting because it is of exactly the same size, and has the same general appearance as that which I showed at the November meeting, but has the magical feature so very imperfectly developed as to make it evident that no intelligent effort was made by the workman to bring it out. The magical image is confused and imperfect, yet so similar in character to that produced by the other mirror as to leave no room for doubt that, although not intelligently developed, it results from substantially the same causes.

There are several peculiarities about this mirror which confirm the explanation that I gave; but it appears that that explanation was already known to some, and so well established as to require no further experimental demonstration. Prof. Wm. A. Anthony, whose lecture on electrical measurements you will remember, assured me that that explanation was made to him in 1881, by Duboscq, of Paris, who then manufactured imitations of the Japanese mirrors, and sold them as curiosities. I doubt if it ever appeared in any publication printed in the English language.

Now, what interests me most in this particular mirror, is the fact that it appears to have been made by a workman who tried to imitate and improve upon the productions of another, and nearly

destroyed the magical feature through his efforts to improve the appearance of the mirror. The design in relief at the back, although showing the same objects, is far more elaborate and detailed than that of the first mirror, and this very elaboration of design, so evidently intended to make the mirror more attractive to the eye, is calculated, both by its character and distribution, to produce the indistinctness which we see in the magical image. The first mirror has broad spaces between those figures which show plainest in the magical image, but this one has the corresponding parts filled up by figures representing clouds, details of foliage, etc., which support the tympanum of the mirror just where it ought not to be supported in order to have the grinding and polishing operations bring out the more important figures so that they will show distinctly in the reflection.

Still another Japanese mirror has been handed in, this evening, by Mr. Outerbridge. It has the same faults as my own, but in a far greater degree. The figures at the back are so closely crowded together that I doubt if any of them will be distinguishable in the reflection.

[A trial of this mirror showed that the magical feature was practically absent.]

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## REPORT OF THE COMMITTEE ON SCIENCE AND THE ARTS ON THE CYCLOSTYLE AND DUPLICATING APPARATUS OF THE PENNSYLVANIA CYCLOSTYLE COMPANY.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, December 1, 1887.

The Sub-Committee of the Committee on Science and the Arts, constituted by the FRANKLIN INSTITUTE of the State of Pennsylvania, to which was referred for examination the

CYCLOSTYLE AND DUPLICATING APPARATUS OF THE PENNSYLVANIA CYCLOSTYLE COMPANY,

*Report*, That the cyclostyle is a species of stylus which perforates a specially-prepared paper, and makes of it a stencil; the stencil is stretched in a frame, and by passing an inked roller over the writing or drawing, a large number of copies can be produced. The details of the process are similar to the duplicating process of

T. A. Edison, the "electric pen" of the latter being replaced by the much simpler implement, called the cyclostyle.

This consists of a minute wheel with teeth, or ridges on its periphery, mounted on an axis carried by a holder or handle similar to a lead-pencil. In writing, it is held the same as a pen or pencil, and lines of minute perforations are made through a thin waxed paper placed upon a hard, plain surface like a tin plate.

The paper used is very thin and tough, and is made from bamboo fibre, and is thinly coated with paraffine wax, so that a letter or word once written on the stencil may be erased by rubbing the wax smooth with the thumb nail, and corrections in the writing can thus easily be made.

The stencil holder or frame is a great improvement over those which have heretofore been in use for the same purpose. Two frames are hinged together; in the sides of one is a groove, and upon the other is a tongue, which corresponds to it, and the two when pressed together clamp the paper so as to stretch it taut and perfectly even like a drum-head. This ensures a clear copy, as the stencil does not move while the inked roller is upon it. Heretofore it has been necessary to fasten the stencil by small clamps, first at the top, then at the bottom, and then at the sides, and the result was seldom satisfactory.

What the inventors, David Gestetner and Augustus D. Klaber, claim as new, are the perforating instrument, the waxed paper and the clamping frame above described.

But little practice is required to enable a person to write quite rapidly with the cyclostyle, and the ease and rapidity with which the stencils can be placed in the frame and copies made from them, take away the principal objection to the use of duplicating processes of this character.

E. ALEX. SCOTT, *Chairman*,
FRED. E. IVES,
H. R. HEYL,
W. M. MCALLISTER.

*January 4, 1888.*

Amended by the recommendation of the award of "THE JOHN SCOTT LEGACY PREMIUM AND MEDAL," and as so amended, adopted.

WM. D. MARKS, *Chairman.*

I certify that the above is a true copy from the records.

WM. H. WAHL, *Secretary.*

# CORRESPONDENCE.

## LIMITING NUMBERS OF GEAR TEETH.

*A Disclaimer by* PROF. MACCORD.

In the last paragraph of Mr. Geo. B. Grant's paper on the "Limiting Numbers of Gear Teeth" (see February issue of this JOURNAL), is found the following in relation to the involute system.

"Thus when recess equals two-thirds of the pitch, we find that the smallest possible pinion has 5,300 teeth, and that no smaller gear will drive anything. This pinion will drive any gear not smaller than that determined by (12), and it will drive any internal gear that will not interfere with it."

So far good; but this foot-note is appended, viz.: "Prof. MacCord states that the rack is the minimum follower for this minimum driver; see section 243 and the tables of section 276 of his *Kinematics*. * * * * * Prof. MacCord claims that I have here misrepresented him; but his language is plain, and will bear no other interpretation than that his tabulated followers are the least possible followers for the given drivers."

| | DRIVERS. | FOLLOWERS. | |
|---|---|---|---|
| Involute System. | **5·30** | ∞ | Recess = ⅔ Pitch. |
| | 6 | 10 | |
| | 7 | 9 | |
| | 9 | 8 | |
| | 12 | 7 | |
| | 28 | 6 | |
| | ∞ | **5·36** | |

The portion of my table to which immediate reference is made is very small, and I here insert it in full. As to the *whole numbers*, the above interpretation of my language is correct, but "section 243" contains no allusion whatever to the fractional numbers (such as 5·30, 5·36, etc.).

No explanation was considered necessary: these numbers, indicating in each case the *least possible driver* and the *least possible follower* for a rack, being printed in a conspicuously distinctive type, I thought, and I still think, their meaning obvious beyond the possibility of misunderstanding.

Reference to section 243 being fruitless, then, the following is found in section 247: "the exact limiting radius of a pinion which can just drive a rack, is 5·58; * * * * * when the driver becomes a rack, the value of this radius is 9·83."

Comment is needless; I, therefore, beg summarily to say that I have never made, implied, nor imagined the statement that "the rack is the minimum follower for this minimum driver," as asserted by Mr. Grant in the foot-note above quoted.

CHARLES W. MACCORD.

STEVENS INSTITUTE OF TECHNOLOGY,
HOBOKEN, N. J., *February 29, 1888.*

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## THE LIMITING DIAMETER OF DESCRIBING CIRCLES FOR TEETH OF ANNULAR WHEELS.

*The Committee on Publication of the* FRANKLIN INSTITUTE.

GENTLEMEN:—It has been called to my attention several times during the last two or three years, that the valuable book on "Kinematics," written by Prof. C. W. MacCord, contains a discussion of "the Limiting Diameter of Describing Circles for Teeth of Annular Wheels," in which he practically claims to be the original discoverer of the law of kimematics bearing on that point. It has occurred to me that it might be appropriate to call attention in your JOURNAL to the fact that you published an article from my pen, covering a demonstration of that law, in your issue of January, 1877.

From the fact that Prof. MacCord's book appeared several years later than this date, it would seem, in the absence of evidence to the contrary, that he was not aware of your previous publication of the matter. At the time of writing, I sent the discussion to you, with fear that what seemed to me to be an addition to our knowledge of laws of mechanics, might be shown in your next number to be old to the world. It is gratifying, therefore, to have such good testimony as the above appears to be to the originality and importance of the discovery. ALBERT K. MANSFIELD.

280 BROADWAY, NEW YORK, *February 13, 1888.*

## OBITUARY.

### JOSEPH EASTBURN MITCHELL.

The subject of this sketch was born in Philadelphia, in the old district of the Northern Liberties, on the 3d of August, 1817. He died on the 5th of October, 1887, in his seventy-first year. His ancestors both on the paternal and maternal side were English, and were long settled in America. His father, JAMES MITCHELL, was, in his day, a prominent master-builder, and a member of the Carpenters' Company of Philadelphia. The father of JAMES MITCHELL, and the grandfather of the subject of this memoir, was BENJAMIN MITCHELL, who married SUSANNAH WILSON, a granddaughter of THOMAS SAY, who is remembered as a distinguished naturalist, and one of the founders of the Academy of Natural Sciences.

On the death of his father, which occurred in 1851, JOSEPH E. became the successor of the firm of JAMES MITCHELL & SON, and carried on the business of importing and manufacturing grindstones and grindstone fixtures (founded by his father, JAMES, in 1810) until his death. As evidence of his careful and systematic business habits, it will be sufficient to say that, notwithstanding his devotion to many public duties, he was never involved in financial difficulties, but, on the other hand, increased many-fold the business that he had inherited from his father. Shortly after assuming control of the business, he erected the large factory at Beach and Green Streets, for the storing and finishing of large grindstones. He introduced, from time to time, substantial improvements upon the fixtures in use, and invented, among other things, a valuable tool for trueing grindstones. His business enterprise was manifested in the notable displays of his manufactures at the World's Fair in Vienna in 1873, and in Philadelphia in 1876. The Tuscan Column, built up of grindstones, will be recalled by every visitor to our Centennial Exhibition, as one of the most cleverly-devised and striking exhibits in Machinery Hall. The collection contained thirty-eight varieties of grindstones, including specimens of all the best known grits of Europe, collected for the purpose by WILSON MITCHELL, his son.

The instructive character of this exhibit, having attracted the attention of the late Professor SPENCER F. BAIRD, then the Secretary of Smithsonian Institution, and the head of the projected National Museum in Washington, he decided to make a collection of the materials used in the arts for grinding purposes as one of the features of the Museum, and, to this end, invited and secured the co-operation of MR. MITCHELL, who entered warmly into the project. In carrying the project into execution, a circular was issued to all manufacturers of grindstones in the United States and Europe, whose addresses could be obtained, inviting each to send to JOSEPH E. MITCHELL specimen stones in the rough to be finished by him, and sent to Washington for permanent preservation in the grindstone section of the National Museum. The scheme also contemplated the preparation of thin sections of the various grits, scythe-stones, oil-stones, whetstones, etc., thus collected, for microscopic study of the texture and other physical qualities upon which the value of these materials in the arts depends. As the direct or indirect result of this effort, the National Museum has come into possession of a large and instructive collection of these materials.

One of MR. MITCHELL'S distinguishing personal peculiarities was his love of fruits and flowers, and the warm interest he felt in everything that contributed to the cultivation of these innocent and refining pursuits was manifested by his active co-operation in the work of the Pennsylvania Horticultural Society and the American Pomological Society. Of the first named Society, he was twice the President, and occupied that office at the time of his death, and he took a prominent part in the biennial meetings of the latter.

In 1873, the Councils of Philadelphia authorized the appointment of a Commission "to represent the City of Philadelphia at the Vienna Exposition, and to collect such information as might aid * * * in carrying out the objects of the Centennial Exhibition." Of this Commission, MR. MITCHELL was appointed the president, and the results of its work appear in a report presented to MAYOR STOKLEY in December of the same year, and which embodied a number of eminently useful suggestions. Of these, it may be interesting to refer to one, which proposed "the erection of a suitable building to receive the mementos which would be

offered at the close of the Exhibition, many of them of great historical interest, and all of them useful as the nucleus for the formation of a Museum of Art, Science and Manufacture." The South Kensington Museum was indicated as the type of this institution, and it was further suggested that in the construction of the Memorial Hall this object should be kept in view. These suggestions, without doubt, originated with JOSEPH E. MITCHELL, and whatever credit may be attached to the formulation of a proposition, which has since assumed concrete form by the establishment of the "Pennsylvania Museum and School of Industrial Art," should properly belong to him. His deep interest in the subject was shown in other ways. He warmly advocated it in an address before the FRANKLIN INSTITUTE, in the course of which he gave an account of the work of the English institution, and described the benefit that would follow the foundation and growth of a similar one in Philadelphia. He was invited, in 1875, to participate in the organization of the Museum, and at the meeting held in pursuance thereof, he recommended the title "Museum of Industrial Art." He was nominated in the same year, when its organization had been completed, to represent this INSTITUTE in the Board of Trustees, but withdrew in favor of the then Secretary of the INSTITUTE. In 1878, he was elected by the House of Representatives to represent the State of Pennsylvania in the Board of Trustees and served in the office by successive re-elections for five years, and manifested his interest in its work by service on the Building Committee (of which, for a time, he was the Chairman), on the Executive Committee, the Committee on Art Industries, and the Committee on Finance.

MR. MITCHELL was one of the corporators of that excellent charitable institution, the "Hayes Mechanics' Home," of which he was also, for many years, one of the managers. He was an intelligent and liberal patron of music, and was one of the originators and guarantors of the Philadelphia Music Festival, and of its outgrowth, the Philadelphia Musical Society or Chorus (still in existence). He was likewise an active member, from its foundation, of the Utopian Club, a local society of professional and amateur musicians.

MR. MITCHELL'S active connection with the FRANKLIN INSTITUTE dates from the year 1867, when he was elected to membership. He became a life member in 1880. He was elected to the

Board of Managers in 1873. In 1876, he was elected one of the Vice-Presidents, which office he occupied down to the time of his death. He served during most of this time as a member of the Committee on Exhibitions. Until within a few years of his death, he was quite regular in his attendance at the monthly meetings, and a frequent participator in the discussions and debates, and his interest in the INSTITUTE was at all times manifested by his advocacy of every measure calculated to increase its usefulness and influence.

MR. MITCHELL was a man of pleasing and dignified presence, and was most affable and courteous in demeanor and in his intercourse with others. His private life was blameless, and those who knew him best will cherish his memory as that of a man animated by a nice sense of personal honor, and by the desire to be useful to his fellow-men.

EDWIN J. HOUSTON, ALEX. E. OUTERBRIDGE, JR.,

THEO. D. RAND,

CHAS. E. RONALDSON, WM. H. WAHL.

## BOOK NOTICES.

SUR LA CONSTITUTION DE LA NAPHTALINE ET DE SES DÉRIVÉS. By F. Reverdin and E. Noelting. Reprinted from the *Bulletin de la Société Industrielle de Mulhouse.* pp. 76, with twenty extensive tables. 8vo, paper, 6 marks. Mulhouse, Alsace: Veuve Bader et Cie.

In drawing the attention of American chemists to this valuable work, a better idea of its aim and scope could not be conveyed than that contained in the first two or three of its opening pages. "Among the hydrocarbons which have attracted the attention of chemists, from a theoretical as well as from a practical point of view, naphthalene occupies certainly a place in the foremost rank. Its substitution products offer numerous cases of isomerism, and under each category the influence of the position of the substituting atoms and atom-groups upon the properties of the compound may be studied in a great number of examples.

"Moreover, within the last twelve years, the derivatives of naphthalene have, in an industrial point of view, assumed an enormous importance.

"In the first phase of the development of the industry of the artificial coloring matters, from the discovery of mauveine, of fuchsine and its derivatives, up to the discovery of artificial alizarine, the primary substances were furnished exclusively by the hydrocarbons of the benzene series. Attempts

had also been made, indeed, to prepare coloring matters by applying to naphthylamine and naphthol those reactions which, with aniline and phenol, had furnished such varied and beautiful bodies, and in certain cases products with tinctorial properties had been obtained, but save binitronaphthol, none of them were ever employed on the large scale.

"In 1869, the synthesis of alizarine attracted attention to a hydrocarbon up to that time almost unknown, anthracene, and made it the starting point of a magnificent industry. It was not until 1874 that naphthalene, in consequence of the discovery of eosine, made its entrance into the domain of chemical industry; serving, as is well known, for the preparation of phthalic anhydride, one of the primary substances employed in the formation of this dye.

"Its consummation in point of importance was not reached, however, until in 1876–1877, the industry of the azo-dyes underwent an unexpected and enormous extension.

"A great number, one can in fact say, the majority of the azo-dyes are derivatives of the naphthylamines, of the naphthols, or of the sulphonic acids of the same.

"The development of the industry of the azo-derivatives of naphthol has become a new demonstration of the importance of the study of isomerism of position, for the practical chemist; while no where else perhaps can one better prove the effect of this isomerism on the tinctorial properties of the bodies. Indeed, not only the colored derivatives of $\alpha$- and of $\beta$-naphthylamine, of $\alpha$- and of $\beta$-naphthol are capable of being sharply distinguished, the one class from the other, but even the isomeric sulphuric acids of one and the same naphthylamine or naphthol give from point of view of their application very unlike results.

"Thus, to cite only a few examples: The compound resulting from the combination of diazophenylsulphonic acid with $\alpha$-naphthol is a brown, with $\beta$-naphthol it is a beautiful and brilliant orange; $\beta$-naphthol under the influence of concentrated sulphuric acid furnishes two bi-sulphonic acids, which when coupled with diazoxylol, the one gives a yellow ponceau, while the other gives a bluish ponceau; from the same $\beta$-naphthol two monsulphonic acids are also derived, producing by combination with diazotised amido-azo-benzol sulphonic acid the one a dull orange, the other a magnificent scarlet. Diazotised toluidine produces, according to the naphthalene derivative with which it is combined, reds, violets and even blues.

"We see from these few facts how the exact knowledge of the isomerism in the naphthalene series can become interesting from a practical standpoint. This knowledge is none the less interesting viewed from the theoretical side. While the benzol series presents only examples of quinones and nitroso derivatives of the para-series, naphthalene affords us the opportunity of studying as well the properties of like derivatives of the ortho-series; the study of ortho- or $\beta$-naphtho-quinone possessing a peculiar interest.

"The published researches upon the naphthalene derivatives are extremely numerous; they are scattered here and there through a great mass of chemical literature, and it is not always easy to form a conclusion as to just how far such and such a class has been studied, and what is the number and constitution of its known representatives.

* * * * "The work which to-day we have the honor of presenting to you, and which shows the actual state of our knowledge of the naphthalene derivatives, is for the most part entirely new, having retained merely the general disposition of material peculiar to our earlier work.

"We shall discuss at the outset the constitution of naphthalene, the number of its isomeric derivatives and the methods for the determination of chemical position.

"Later on we give in the form of a series of tables all the known derivatives, indicating in few words the preparation, and giving the bibliography complete. With this arrangement, it will be easy for anyone who may wish to make a thorough study of certain categories of derivatives to refer directly without loss of time to the original memoirs. In many cases, from the preparation of a derivative, we are already in a position to draw conclusions relative to its constitution, while in other cases its transformation into a derivative of known constitution serves to establish the same. In many cases the structure has not yet been determined with perfect precision, or may even be altogether doubtful; when this is so, and in general where it appears to us advantageous, we shall discuss the formulæ in referatory notes annexed to each table."

Coming, as this work does, from the hands of those who not only occupy prominent places among the leaders in pure chemical research in Europe, but who, at the same time, from their eminent familiarity with the application of the results of this research in the chemical industry, are able also to speak with authority with reference to both sides of the subject, this book deserves to meet with a cordial reception amongst those who would advance our knowledge in this fruitful and fascinating field, as well as at the hands of those who are engaged with the industrial application of the principles of organic chemistry, to which latter class it cannot fail to prove a valuable aid.

A. G. P.

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NEW CROTON AQUEDUCT OF NEW YORK. Report of the Aqueduct Commission, 1883 to 1887. 4to. New York City, January 1, 1887.

It rarely happens that the American engineer has either the time or money at his disposal for the preparation of elaborate monographs of extensive public works, and his experiences are therefore lost to the profession. In some cases the records are unearthed by foreign engineers and published in their technical journals, and thus the native engineer may learn something of the labors of his colleagues.

Hence it is that a publication such as that before us attracts so great attention, and is so eagerly sought after by the profession. To say that the report is complete gives but a feeble idea of its contents. The great value of the works consists not only in the statistics of precipitation, progress, cost, discharge, etc., but also in the graphical diagrams showing these data at every stage and condition of the work, and the beautiful phototype illustrations which so handsomely embellish the work and give it a practical value to the non-technical reader. The volume contains also complete working

drawings of the tunnels, shafts, syphons, reservoirs, dams, valves, gate-houses and other accessories, rendering it invaluable to a large class of constructors. L. M. H.

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THE VOSBURG TUNNEL. By Leo Von Rosenberg, 35 Broadway, New York. 55 pp. 1887. 4to paper.

This is a special report upon a cut-off tunnel on the Lehigh Valley Railroad, in Wyoming County, Pa., whereby a bend of five miles is traversed by a double track tunnel of about 3,900 feet. The report is profusely illustrated, showing methods of conducting the work, progress, machinery, timbering, cost, dimensions, etc., and forms a very valuable contribution to the literature of the subject. L. M. H.

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AMERICAN INVESTMENTS. Some Suggestions as to Causes of Existing Depression in Trade and their Remedy. Wm. L. Breyfogle, Louisville, Ky. 1886. *Courier Journal* Printing Company. pp. 36.

This brochure is important, not only for the information and recent data it contains, but as an able argument in favor of developing the resources of the South. The author remarks as to the latter: "It is, in truth, wonderfully rich in all the gifts of Nature, climate, soil, water-courses, water-power, timber, coal, iron, clay, stone—all the metals and minerals of common use to the world. It is within bounds to say that it has double the foundation of natural wealth on which to build an empire that the West had." After mentioning the business depression prevailing in the United States for several years up to 1885–86, and showing the great advantages the country possesses over Europe in its unity and not needing an expensive standing army or series of armies, the author says: "The North and South must now become either allies or rivals. If the first, both sections will gain unprecedented prosperity. If the last, then while the North must lose much, the South will gain something, but far less than if both sections move together. In other words, the South can grow in a way to build up the North, or it can grow at the expense of the North. Nothing can prevent its growth, but it is for the North alone to determine what share or part she will take in Southern development."

What follows are mainly proofs of this proposition, furnishing much valuable information and tabular data respecting the principal productions of the United States, and those of the South especially. This pamphlet has a historic as well as economic value, and the great advantage that the data are four years later than those of the tenth census. N.

## SCIENTIFIC NOTES AND COMMENTS.

### CHEMISTRY.

THE FORMATION OF ALLOYS (*Science*, **11**, 100).—William Hallock, of the United States Geological Survey, presented, at the meeting of the Philosophical Society of Washington, on February 18th, an interesting paper on the formation of alloys. In seeking an explanation of the results announced by W. Spring (*Berliner Berichte*, **15**, 595), who succeeded in obtaining certain alloys by subjecting a mixture of the constituent metals in fine filings to a pressure of 7,000 atmospheres, the author conceived the idea that if at any point the constituent metals were in contact, a minute globule of the alloy would be formed. If, during the compression, or subsequently, the temperature of the mass were raised to the fusing point of the alloy, this globule would melt and act as a solvent on the remaining mixture until the fusion of the whole mass. If this idea were correct, the same result could be obtained without pressure by giving more time and an appropriate temperature to the mixture of metals.

The filed metals required for Wood's alloy were mixed in the proper proportions and packed into a glass tube sealed at one end, no greater pressure being exerted than could be produced by a piece of one-eighth-inch wire held between the thumb and forefinger. The tube was suspended in a water bath at a temperature of 98–100° for eighteen hours, at the end of which time the filings had settled considerably. Tapping the tube on the table settled them still more, and in an hour the whole mass was fused. Tin and lead so treated melt at 200°, tin melting only at 230°. Sodium and potassium fuse to the liquid alloy at ordinary temperatures when clean-cut surfaces of the two metals are pressed together.

Thus "an alloy can be formed out of the constituents at a temperature above the melting point of the alloy, although it be far below that of any constituent, with no (appreciable) pressure." W. H. G.

"α" OXY-NAPHTOIC ACID AS A DISINFECTANT (*Pharm. Centralhalle, Dec. 8, 1887.*)—This acid, frequently referred to of late in periodical literature as a new compound, was discovered already twenty years ago by Eller, and the formula $C_{11}H_8O_3$, graphically expressed

$$C_{10}H_6\begin{cases}OH\\COOH,\end{cases}$$

correctly given by him. Eller proposed for it the name carbo-naphtolic acid, but did not separate that obtained from α-naphtol from that gotten from β-naphtol. This separation was shortly after effected by L. Schaeffer (*Ann. der Chem. and Pharm.*, **152**, 291. 1869). He found that the α-acid gave a blue color with ferric chloride, while the β-acid gave a violet-black inky color.

The α-acid fuses at 185° C. The β-acid, obtained with greater difficulty, and fusing at 235° C., was studied more fully by Battershall (*Ibid*, **168**, 121) in 1873, whose results were confirmed by Strumpf (*Ibid*, **188**, 4) in 1877.

Of the innumerable naphtol derivatives obtained since that time, these acids have specially excited practical interest because of their strong antiseptic properties. The firm of "Heyden, successors," obtained a German patent (No. 38,002) on the 8th of July, 1886, for the use of carbon dioxide under pressure, at a temperature of 120° to 140° C., upon the alkali salts of *α*- and *β*-naphtol. The discoverer of this process (so similar to the salicylic acid process) was R. Schmitt, who, with Burkard (*Ber. der deutsch. chem. Ges.*, 20, p. 2699), described a whole series of derivatives of these carbonaphtoic acids.

On account of its greater stability, the *α*-acid has hitherto been alone considered, and this, by the new method, can be made very cheaply (the kilo costing, at wholesale, according to purity, from six to ten marks, or $1.44 to $2.40). It forms needle-like colorless crystals, which are difficultly soluble (1 to 30,000) in pure cold water. The odor resembles that of naphtol, and excites sneezing. When carefully heated, the acid sublimes unchanged. As before mentioned, the solution is turned blue by ferric chloride, but with a shade of green. If the acid is pure, the solution keeps unchanged; the commercial acid, however, changes in sunlight, first to a yellow and then red. At a boiling temperature, naphtol is formed in the solution, while carbon dioxide is liberated. The ethereal, alcoholic and other solutions as yet need not be practically considered, nor any of the salts which are prepared with difficulty and are mostly only slightly characteristic. The crude acid contains 0·4 per cent. of ash, consisting of lime, iron, alumina, chlorine, sulphuric acid, etc. The antizymotic action of *α*-naphtoic acid, according to a statement of Holmes (*Pharmaceutical Journal*, thirty-second year, p. 662, November 19th, 1887), is five times stronger than that of salicylic acid. Though we may hesitate to express such differences of effective strength by definite numbers, we may readily convince ourselves of the greater power of the new disinfectant by making an experiment with blood. If, for example, we add a few decigrams of salicylic acid to fifty centimetres fresh bullock's blood, putrid decomposition is not materially arrested, while with an equal addition of *α*-oxynaphtoic acid, the odor of putrefaction will not develop for weeks. Still more striking is the antiseptic action when urine is taken.

This property of the acid, in connection with the fact that it can be manufactured so cheaply, has attracted the attention of students of hygiene and veterinary surgeons. The publication of a series of experiments concerning its physiological and germicide qualities, by Lübbert and several veterinary institutes, is shortly expected to appear in the technical journals. Unfortunately the general use of the substance is rendered more difficult on account of its slight solubility in water. Likewise its use in preserving articles of food will be precluded by its poisonous character. On the other hand, its poisonous effects will not hinder its use as an antiseptic in surgery in the light of the experience gained with corrosive sublimate.

It will, perhaps, replace iodoform, which has lately been called into question, its solubility in ether making it possible to obtain a stable half per cent. collodion solution.

The *α*-oxynaphthoic acid will have possibilities in the following cases: To keep paste and similar materials odorless, to disinfect water closets, etc., in

fact in all instances where powder-like disinfectants may be used, and lastly in cases where sublimate is excluded on account of the presence of albumenoid matter. S. P. S.

A NEW SOURCE OF GERMANIUM.—Gerhard Kruss (*Berl. Ber.*, **21**, 131) has found germanium in the acid residues from the treatment of euxenite, and by working up a kilogramme of the residue obtained by extraction of euxenite with potassium acid sulphate, he was able to identify the element beyond doubt by the preparation of its sulphide, oxide and chloride, besides the isolation of the metal itself, comparison being made directly with the oxide of germanium prepared by Winckler and furnished by him. The discovery is of little more than theoretical interest at present, for the proportion of germanium in euxenite does not exceed one-tenth per cent. It seems probable that the element may be found in other titaniferous minerals, and experiments for its discovery are to be undertaken on rutiles, yttrotitanites, wöhlerites, etc. W. H. G.

ON THE REGULAR GENERATION OF GASEOUS HYDROGEN PHOSPHIDE.—J. Messinger and C. Engels (*Berl. Ber.*, **21**, 326) have found that phosphine is produced regularly and easily by the action of aqueous ether on phosphonium iodide. The ether of commerce contains sufficient water, and the operation is conducted satisfactorily in a small Kipp's apparatus; five grammes of phosphonium iodide will produce a current of the gas for an hour. The hydriodic acid formed at the same time is entirely retained by the ether, forming a layer which is insoluble in the latter liquid, and collects at the bottom of the apparatus. W. H. G.

THIOCARBONYL CHLORIDE (*Berl. Ber.*, **21**, 337).—This compound $CS Cl^2$, formerly difficult to prepare, may now be obtained in quantity in commerce, being manufactured for technical purposes. Henry Bergreen has examined the commercial product and describes that sent out by Kern and Sandoz, of Basle, as a red, mobile liquid, slowly decomposed by atmospheric moisture, and fusing in the air. Its odor is penetrating, attacking the mucous membranes and producing a sensation of sweetness. It boils between 68° and 74° C. It contains a proportion of carbon tetrachloride from which it is scarcely possible to separate it by distillation. W. H. G.

HYDROQUINONE AS A DEVELOPING AGENT for gelatine plates has passed the experimental stage, and it remains only to cheapen it to insure a large demand. The advantages it presents over pyrogallol, especially to amateurs, appear to be in the simplicity of the developer, its keeping qualities, and the possibility of continuous use of the same solution for weeks, whilst the quality of the negatives is claimed by some to be superior to those by pyrogallol in some respects. The following formula is given by Dr. Piffard:

| | |
|---|---|
| Hydroquinone (Merck's), | 100 grains. |
| Carbonate of potash, | 300 grains. |
| Sulphite of soda (crystals), | 400 grains. |
| Water, | 20 ounces. |

Mix and filter, and, after use, return to the bottle for future use. It is adapted to varied exposures from flash light to time exposures. A weaker developer is highly recommended by Mr. Carbutt for lantern slides, and for

making intense negatives from black and white drawings for photo-mechanical processes, on account of the clearness of the shadows with great density of the high lights obtainable by it. The tone of the transparencies is also said to be very agreeable, and the quality from a thin or over-exposed negative better than can be obtained with ferrous oxalate.

As a still further simplification, M. Balagny recommended very highly to the Photographic Society of France, the immersion of exposed plates, whatever the nature of the subject, in a vessel containing a large quantity of developer and removing as they may be developed. The developer recommended, consisted of 100 parts of a twenty-five per cent. solution of sulphite of soda, 200 parts of a twenty-five per cent. solution of carbonate of soda, and twenty parts of a ten per cent. alcoholic solution of hydroquinone. C. F. H.

EXPLOSION OF ETHER LIME-LIGHT APPARATUS.—The British *Journal of Photography*, March 2, 1888, gives an account of the explosion of an ether saturator while in use with a pair of dissolving lanterns. The lanterns were overthrown, operators bruised, and flaming ether was thrown about the room, burning furniture, etc. The saturator was in the form of a tank containing liquid ether, over the surface of which the oxygen was made to pass by a circuitous route. Other serious accidents have resulted from the use of this form of ether saturator, and the dangers of its use have been repeatedly pointed out and warned against. The fatal defects of this saturator are: first, that the passage for oxygen is so very large (growing larger with the consumption of the ether) that if, from any cause (such as the too rapid vaporization of ether in a cold room) the mixture becomes explosive, its bulk is sufficient to produce a violent explosion; second, that no matter how little ether there may be in the tank, it is sure to be thrown flaming about the room if such an explosion does occur.

The continued use of this dangerous piece of apparatus seems all the more surprising in view of the fact that an equally efficient and absolutely safe saturator has been on the market for several years, which is used in this country to the exclusion of every other form, and is fully endorsed by experienced lantern operators and men of science who have used it.*

In the Ives' saturator, the passage for oxygen is very small, and remains of the same size at all times, so that it is impossible to produce an explosion that will either burst or injure the saturator; and when the ether supply is low enough to permit of the production of an explosive mixture, it is impossible to throw any of it out by an explosion. Even this harmless explosion cannot occur so long as operators follow published instructions. F. E. IVES.

FORMATION OF SUGARS FROM FORMALDEHYDE (*Berl. Ber.*, **21**, 270).—O. Loew has obtained sugar-like substances by boiling solutions of formaldehyde with lead, iron and tin, lead acetate and certain organic substances. The substances appear to be of the nature of formose; their hydrazine compounds have about the same fusing points as those obtained from formose, and yield analytical results showing a corresponding composition. W. H. G.

* For a description of this apparatus, see this JOURNAL (**125**, 28, January, 1888).—W.

MAKING COPPER TUBES BY ELECTRO-DEPOSITION.—*Modern Light and Heat*, **4**, 324, gives us the following description of an interesting method of obtaining copper tubes by an electrolytic process, which has recently been devised by Mr. W. Elmore, of England. By this method tubes having a tensile strength of from fifty to 100 per cent. in excess of brazed tubes are easily made. The process is very simple and consists in depositing the copper on an iron cylinder which is constantly revolving in the bath at a fixed rate of speed. An agate burnisher, which moves slowly from end to end of the cylinder, much on the plan of the tool of a screw-cutting lathe, rubs down the fine crystals of copper, matting them together. When the deposit is sufficiently thick the cylinder is taken from the bath and subjected to the temperature of superheated steam, when the sudden expansion of the copper tube frees it from the iron core, and it can be removed. The breaking strength of such tubes is said to be from twenty-seven to forty-one tons per square inch. W.

A DELICATE TEST FOR BISMUTH. E. B. Stone (*Jour. Soc. Chem. Industry*, **6**, 416).—When a dilute solution of bismuth sulphate, containing but little free sulphuric acid, is treated with a few drops of concentrated potassium iodide solution, a beautiful yellow color is produced, and is sensible with 0·00001 gramme bismuth oxide in ten cubic centimetres of solution. On account of the uncertainty which follows the similarity of the color to that which would be produced by the separation of a little iodine, Dr. Watson proposes that a little solution of lead nitrate be added to the neutral or only faintly acid liquid before adding the potassium iodide. The mixture of precipitated lead and bismuth iodides then has a color varying from orange to deep red. By careful operation, less than a millionth of bismuth in solution may thus be detected. W. H. G.

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## ENGINEERING.

DIGEST OF A REPORT ON CURRENT METER OBSERVATIONS, Mississippi River, near Burlington, Ia., 1879, by G. A. Marr. (Prepared by G. L. Martin, Department of Civil Engineering, University of Pennsylvania).

In 1878, Major F. U. Farquhar, U. S. E., was instructed to examine the Mississippi River above the mouth of the Illinois, for the purpose of forming a plan for the improvement of the low water navigation of this river and that of the Missouri. The portion of the river specially selected for study was between Burlington and Montrose. At the same time and place, a series of observations with current meters, with a specially constructed chronograph, was undertaken. The work was inaugurated by Major Farquhar, with Capt. B. D. Greene, U. S. E., in charge of the field observations for a portion of the time, but Mr. G. A. Marr, Assistant Engineer, "who submits the final report, was in immediate charge of the field work, and has prepared the tables and plates showing the results."

The apparatus used consisted of seven current meters and a chronograph, which was constructed under the direction of Major Farquhar. The chronograph was provided with eight pens working independently, so that six could

be used for current meters, giving an independent and complete record for each, while at the same time the two outside pens could be connected in one circuit for a time record by "being connected with a break circuit chronometer, and marking intervals of one second on each edge of the sheet." Instead of using a single meter and lowering it to different depths, six meters were used simultaneously as "on account of the great fluctuations in velocity, not only from the surface downward, but also in the whole volume of the water, it appeared that a system which would give simultaneous observations at several distinct points in a vertical plane would give much more reliable and valuable information than the use of a single meter at different depths."

For determining the current velocities, the current meters were held in "position at their various depths by a wire rope," the foot of which was held by a mushroom anchor weighing 150 pounds. The six meters used for the observations were placed at equal intervals from the surface to the bottom of the river.

### RATING THE METERS.

The meters were frequently tested, and for this purpose a frame was prepared, which was held in a horizontal position and at right angles to the direction of the current. The meters were placed in the frame and carefully set at the same height. This gave a comparison of the meters at the same depth, and to eliminate errors due to lateral variations in the current, the meters were reversed on the frame. Arrangements were also made for comparing the velocities as determined by meters and by floats. In the greater number of the observations, the floats were tied at mid-depth. From a study of the results, it was found that the velocity was not uniform over different sections, and hence a comparison of the velocity of the float with that of the meter located just below the section could not be expected to give definite results.

From the chronographic sheets, which give a record for the whole six meters, the velocities for each minute is given and the curve plotted graphically. The depths and the observed velocities at these depths are taken as the co-ordinates and the curve drawn, from which the velocity at any point may be scaled. The mean average velocity of the water at the different depths of the same section of the river is obtained by averaging the mean vertical velocity curves for the different minutes. Thus, on October 7th, at the section having a depth of 19·5 feet, nineteen different minute velocities are obtained for each meter. The final curve is drawn from the means of these.

### RATIO OF MID-DEPTH TO MEAN VELOCITY.

The special object in view in obtaining these results was to find the ratio between the velocity at mid-depth and the mean velocity. To do this, the velocity at mid-depth was taken from the plotted curve. The mean velocity was obtained from averaging the velocities at a large number of depths. For this purpose the results are taken from the mean vertical velocity curve for each 0·4 of a foot in depth. Humphreys and Abbot's formula, giving the ratio of mid-depth to mean velocity, is as follows:

$$V_m = V\tfrac{1}{2}\, d - \tfrac{1}{12}\,(b\,v)^{\frac{1}{2}},$$

in which

$$b = \frac{1{\cdot}69}{(D + 1{\cdot}5)^{\frac{1}{2}}}$$

and $V_m$ is the mean velocity, $V^{\frac{1}{2}} d$ is the mid-depth velocity, $D$ is the depth and "$v$ the mean velocity of the whole cross section of the river." We now see what time and labor would be saved if a number could be determined which would express the ratio. In the equation, "$v$" is needed and it is difficult to obtain. In the present case it was found from "the mean velocities as determined on cross section I." These velocities were plotted and "from the plot the value of $v$ could be taken very closely for any stage." The gaugings referred to were taken at the same time as the current meter observations. Thus, it is seen, the same opportunity for error offers itself in this case as in the other. The difference can be seen from an inspection of the annexed table, which is a small portion of one of the most important ones given in the report. To it has been added a column for the same day, so that a comparison might be made of the results derived from Humphreys and Abbot's formula and those obtained by using the coefficient. In both cases the mid-depth velocity used is the same, being that obtained from the current observations. There have also been added two columns not in the report. The first is the mean velocity taken from the plates for the depths corresponding to ·6 of the total depth, and the second shows the difference between the mean velocity and that obtained by taking ·6 of the depth.

INVESTIGATION AS TO RELATIVE ACCURACY OF DETERMINING MEAN VELOCITY BY A SINGLE OBSERVATION AT PROPER DEPTHS.

For obtaining the ratio of mean to mid-depth velocity, twenty-five results were obtained from the observations, giving a mean coefficient of ·958 with a probable error of ·029. If it were possible to obtain a ratio of the depth of mean velocity to the whole depth with but a slight probable error, it would again greatly simplify the work. From the present investigations, the probable error for the latter is greater than for the former, being ·071, the ratio of $\frac{D_m}{D}$, or of depth of mean velocity to total depth is ·622. Yet it might be possible to obtain sufficiently accurate results by taking the mean velocity as that found at a distance from the surface equal to ·6 of the total depth. For this purpose the two additional columns have been prepared. From an inspection of the twenty-five results it is found that the coefficient ·6 gives results in some cases closer to the true velocity than those given by ·958. The extremes are, for ·6, ·075 and for ·958, ·059. It will be seen, therefore, that a sufficiently accurate result for most purposes can be obtained by taking the mean velocity as that given by the meter when placed at a depth from the surface equal to ·6 of the total depth.

For the purpose of giving some definite knowledge concerning the question of velocities, the experiments were worthy of the trouble which Mr. Marr took to obtain the statements, but it is hardly possible to undertake such careful observations in every case. The circumstances and phenomena connected with the matter are so variable that the results can never be obtained with the desired degree of accuracy at any one point.

TABLE.—COMPILATION OF RESULTS FROM PLATES OF FINAL PLOTTED CURVES.

$V_{\frac{1}{4}d}$ = mid-depth velocity. $V_m$ = mean velocity. $w$ = depth of mean velocity.

| Date. | Stage. | Depth ($D$). | $V_{\frac{1}{4}d}$ | $V_m$ | $V_m$ derived from $V_{\frac{1}{4}d}$ H. & A. formula. | $V_m$ derived from $\cdot 938\ V_{\frac{1}{4}d}$ | $w$. | $\frac{V_m}{V_{\frac{1}{4}d}}$ | $\frac{w}{D}$ | $V_m - \cdot 938\ V_{\frac{1}{4}d}$. | $V_m - V_m$ from $\cdot 6\ D$. | $V_m$ from $\cdot 6\ D$. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1879, Oct. 14. | 0·89 | 13·2 | 2·625 | 2·521 | 2·533 | 2·515 | 8·08 | 0·960 | 0·608 | + 0·006 | . . . | 2·536 |
| | | 13·2 | 2·633 | 2·599 | 2·561 | 2·583 | 7·80 | 0·987 | 0·590 | 0·037 | 0·014 | 2·545 |
| | | 12·9 | 2·604 | 2·581 | 2·532 | 2·495 | 7·80 | 0·973 | 0·605 | 0·038 | 0·006 | 2·538 |
| | | 13·0 | 2·629 | 2·568 | 2·557 | 2·549 | 7·50 | 0·977 | 0·577 | 0·049 | 0·030 | 2·549 |
| | | 13·0 | 2·624 | 2·559 | 2·552 | 2·514 | 7·56 | 0·975 | 0·582 | 0·045 | 0·010 | |
| Mean of 25 observations, . . . . . | | | | | | | | 0·958 | 0·622 | | | |

L. M. H.

THE SIMONDS ROLLING MACHINE.—Fitchburg, Mass. (*Boston Herald* of Friday, February 24, 1888.) This unique machine is said by mechanical experts to be "the grandest mechanical invention of the age," marking a "new era in the manufacture of iron and steel." It is briefly described as a machine in which two flat surfaces, acting vertically or horizontally, and moving in opposite directions, with adjustable dies fixed on them, roll in one motion a piece of metal of regular or irregular shape, and in almost any pattern desired.

The work is perfectly accurate and very rapidly done. It is as if one took a red-hot steel bar and inserted one end of it in the machine and by

one stroke obtained the desired form, say a perfect sphere, or a conical shot, a chain screw or a bolt with thread, head and all complete, a car axle or a tiny calk for lumbermen's shoes; spindles or taper pins. For a vast number of articles it supersedes the work of the lathe, trip-hammer, planer and other machines requiring much loss of time and waste of material, for instead of cutting it away, it produces the proper form and size by compression and thus increases the strength of the product.

The invention is the outcome of a discussion upon a method of manufacturing a bolt, in which Mr. Geo. F. Simonds used, for illustration, a pencil rolled between the palms of the hand, and this was the germ which has been developed into the wonderful machine now running at the Simonds Brothers & Co.'s works at Fitchburg, Mass. L. M. H.

## Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, March 21, 1888.*]

HALL OF THE INSTITUTE, PHILADELPHIA, March 21, 1888.

Vice-President, W. P. TATHAM, in the Chair.

Present, 132 members and fifty-two visitors.

Accessions to membership since last meeting, two.

Mr. FRED. E. IVES read a description of his suggestions for taking color photographs, and exhibited a specimen picture with the lantern.

Mr. A. O. GRANGER gave an oral description of the incandescent gas burner, adapted for non-luminous gas of any description, invented by Auer v. Welsbach, and exhibited a number of the burners in operation.

Mr. MORRIS P. JANNEY, of Easton, described and exhibited the operation of an automatic "Electric Water-Level Indicator" for steam boilers.

The above communications have been referred for publication.

Dr. WM. H. GREENE exhibited and described (for Messrs. Jas. W. Queen & Co., Agts.) some improved forms of fine balances.

The Secretary's report embraced the description and exhibition of a self-winding clock, on behalf of Messrs. Breitinger & Kunz, of Philadelphia, and of a number of specimens of the new product called Vulcabeston, made by the Johns-Pratt Company, of Hartford, Conn.

The presiding officer announced in suitable terms the death of Prof. JAS. C. BOOTH, for many years an active member of the INSTITUTE and of its staff of instructors.

Mr. S. LLOYD WIEGAND followed with a eulogy of the deceased, in which he specially referred to his admirable qualities as a teacher. He moved that a committee be appointed to prepare an appropriate memorial for publication. The motion was carried.

The President appointed Messrs. S. Lloyd Wiegand (Chairman), Chas. M. Cresson, M. D., and Alex. E. Outerbridge, Jr., as the Committee.

Adjourned. WM. H. WAHL, *Secretary*.